# HISTORY OF THE OFFICE OF SCIENTIFIC RESEARCH AND DEVELOPMENT

A summary of the activities of the entire organization in the development of improved weapons of warfare has been published as *Scientists Against Time* by James Phinney Baxter, 3rd. Details about the different parts of the organization are presented in a series of volumes with the common title, *Science in World War II,* which has been prepared under authority from:

Vannevar Bush, President, Carnegie Institution of Washington
Director, Office of Scientific Research and Development

James B. Conant, President, Harvard University
Chairman, National Defense Research Committee

Alfred N. Richards, Vice-President in Charge of Medical Affairs, University of Pennsylvania
Chairman, Committee on Medical Research

Karl T. Compton, President, Massachusetts Institute of Technology
Chief, Office of Field Service

## *Science in World War II*

NEW WEAPONS FOR AIR WARFARE
DIVISIONS 4, 5, AND 7 OF NDRC; SECTION T, OSRD

COMBAT SCIENTISTS
OFFICE OF FIELD SERVICE; NALOC; DOLOC

ADVANCES IN MILITARY MEDICINE
COMMITTEE ON MEDICAL RESEARCH

ROCKETS, GUNS AND TARGETS
DIVISIONS 1, 2, AND 3 OF NDRC

CHEMISTRY
DIVISIONS 8, 9, 10, 11, 19 AND TDAC OF NDRC

APPLIED PHYSICS: ELECTRONICS; OPTICS; METALLURGY
DIVISIONS 13, 15, 16, 17, 18 AND COMMITTEE ON PROPAGATION OF NDRC

ORGANIZING SCIENTIFIC RESEARCH FOR WAR
ADMINISTRATIVE FRAMEWORK OF OSRD

# ROCKETS, GUNS AND TARGETS

SCIENCE IN WORLD WAR II

Office of Scientific Research and Development

# Rockets, Guns and Targets

Rockets, Target Information, Erosion Information, and Hypervelocity Guns Developed during World War II by the Office of Scientific Research and Development


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## *PUBLISHER'S NOTE*

Under the terms of the contract for the publication of *Rockets, Guns and Targets* and of the other volumes in the long history of the activities of the Office of Scientific Research and Development, entitled *Science in World War II,* the publisher has agreed to waive its right under the copyright of each separate volume after ten years from the date of its publication. Thereafter the volume in question will be in the public domain and dedicated to the public.

This history of OSRD is intended to be a survey of the way in which certain weapons were developed, improved, or brought into use, and is not intended to be a complete documentary record of the making of the inventions that happen to be mentioned in it. Therefore the dates used, which serve to outline the general chronology, do not necessarily establish dates of conception of inventions, of their reduction to practice, or of occasions of first use. Moreover, in the interest of simplicity, credit for the development of a device is frequently given to a group identified by the name of its leader, rather than to the individual who was the actual inventor.

The authors and editor of this volume receive no royalty from its sale.

# THE AUTHORS

The histories in this volume were prepared by the several authors under the direction of the Chiefs of the respective Divisions of NDRC. The Chief of each Division has assumed responsibility for the material included in his portion of the volume.

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*Chief:* Frederick J. Hovde, President, Purdue University.

*Authors:* Bayes M. Norton, Professor of Chemistry, Kenyon College; Technical Aide of Section L, Division 3 from 1943 to 1946. William Huse, Professor of English, California Institute of Technology; Historian for Cal. Tech. Contract OEMsr-418. Clarence N. Hickman, Physicist, Bell Telephone Laboratories; Chairman of Section H, Division A from 1940 through 1942 and Chief of Section H, Division 3 from 1943 to 1946.

# FOREWORD

THIS volume of the history of the Office of Scientific Research and Development, describes the work of three of the nineteen divisions of the National Defense Research Committee. Two of these three divisions (1 and 3) were concerned with one of the oldest and most fundamental of all the problems of military technology, that of the propulsion of missiles against the enemy, a process accomplished in modern warfare either by gunfire or by rocket power. The other (2) was concerned with the equally fundamental problem of terminal ballistics, that branch of military science which studies the behavior and effect of missiles when they arrive at the enemy target.

The volume was written by the authors listed on page ix, under the direction of the division chiefs named, and was edited by Professor John E. Burchard, onetime Chief of Division 2, NDRC, and later Deputy Chief of the Office of Field Service, OSRD. Reading the volume over — in partially completed manuscript form — it is a satisfaction to find that the authors and editor are accomplishing their assignment both competently and interestingly. Not only does the volume give us an adequate account of the many projects that had to be undertaken, but it also gives correct notions for the physical principles involved in the production and employment of weapons, and provides some appreciation of those difficulties — physical and human — which delay the progress of research and development in war. Once more we recapture in memory the days of anxiety and urgency, the days of emergence from confusion into organization, the days of exhausting toil, and the final days of satisfactory accomplishment.

Part One of the volume describes the NDRC development of rockets. Such weapons were used by the Chinese against the Mongols as early as 1232, but since that time have had only sporadic military use until the present war, when they were employed in great numbers and variety by all the major military powers. There was an early appreciation in NDRC — even before our knowledge of the rocket developments of those countries that later became our allies or enemies — that the novel character of this weapon made it especially appropriate for NDRC investigation. Rockets for many different purposes were developed under NDRC auspices, and produced and used in enormous quantities. They ranged in size all the way from the Bazooka (an Army Ordnance development, with NDRC help), which could

be shot from the shoulder of an infantryman, with deadly effect against tanks because of its special head, to Tiny Tim, a foot in diameter and weighing twelve hundred pounds, which could be fired from airplanes with safety, accuracy, and tremendous effect. The close co-operation of the Navy with one of the academic contractors of the NDRC rocket division should be specially emphasized. Here there was an immediate give-and-take, with rockets developed and put into crash procurement to meet the changing needs of defense or offense. Rockets played a great role in reducing casualties and in shortening the war; their development by NDRC was an outstanding success.

Part Two describes NDRC work on the effects produced by weapons through impact and explosion. At the start this work was in part supplementary to that of the National Academy Committee on Passive Protection Against Bombing, which was carried out under interlocking direction. By the end of the war exhaustive studies had been made — on the protection against bombs or projectiles afforded by concrete, by armor plate, by earth, by plastic armor, and even by spikes on a tank — on the penetrating powers of bombs, of standard projectiles at conventional velocities, and of hypervelocity projectiles of special design — and on the damage produced by explosions in air, in earth, in water, in concrete, and in fabricated structures. The experimental and theoretical work on the effect of height of explosion on damage by air blast was especially important. The division in charge was a provider of information rather than a constructor of weapons; in the early days this information was mainly concerned with means of defense, but as the war progressed the work of the division was more and more needed in the planning of offense. In addition to many special studies on problems of offense, the division was called on to undertake the training of special personnel for service with the bomber commands. Although not a producer of weapons, the division did supply some useful measuring apparatus, and participated in the development of the "frangible trainer bullet," which has a surprising story told in Chapter XXX with appropriate emphasis. In Chapter XXIX under the heading "Sand in the Gears," there will be found a narration of administrative obstacles encountered by the division. This is an instructive chapter, since it is written without animus, describes difficulties characteristic of wartime administration, and shows how they are to be avoided, endured, or overcome.

Part Three describes NDRC investigations in the field of gun design and construction. Guns, unlike rockets, have long been a conventional military weapon, with necessarily elaborate and well-worked-out techniques of design. Thus, there was an early realization that NDRC must give its help, not on the usual problems of gun design, but in the investigation of some fundamental limitation on gun performance. Hence, as soon as it was appreciated that the possible performance of guns is limited not by their

strength but by their life under fire, an NDRC section was established to study the problems of erosion and hypervelocity. In one of its lines of endeavor, this section, which later became a division, developed sabot projectiles and tapered-bore guns. These devices give a solution of the problem of hypervelocity without undue erosion, by retaining gun bore and powder charge as in a conventional gun, and obtaining increased velocity by a final reduction in the size of the projectile that flies through the air. Such weapons were used by the British and Germans, but were never put into production by our own Ordnance Department. In its main line of endeavor, the division made fundamental studies on the mechanism of gun erosion, for many years a field of previous interest and speculation, but of very slow previous progress. As a result of these studies a variety of methods, applicable under different circumstances, were successfully developed for protecting the bore of gun barrels. These included nitriding, chromium plating, the prerifling of projectiles, a device called the Fisa protector, and the insertion of resistant liners. The development of alloys suitable for the construction of liners to meet one or another Service condition was especially important. The work of the division gave such a satisfactory solution of the problem of machine-gun erosion, that machine-gun barrels were flown to the Pacific. It also provided the necessary basis for the ultimate construction of large-caliber hypervelocity guns.

The reader will find this volume interesting and instructive. If another war comes we must hope that the lessons which it teaches will prove helpful. Most of all, however, we must hope that the affairs of men will be so organized, and the spirit of men so directed, that these lessons, though still instructive, will not be needed for the prosecution of another war.

RICHARD C. TOLMAN
*Vice-Chairman, NDRC*

# PREFACE

THIS book does not have unity. As Dr. Tolman has stated, it tells the story of three divisions of NDRC, which had only tangential interests one to the other.

The common factor was that the three divisions operated under a single house flag, that of OSRD, and that they all, therefore, conformed in theory to certain common principles of administration. In practice there were deviations here as well.

Under the circumstances it has appeared that any effort at a uniform presentation would do more harm than good; it has seemed wiser to me to confine myself to the simple role of the man who had finally to drive the writers of the words into the publisher's corral; this and the establishment of certain minimum uniformities of technique in footnotes, appendices, identifications, plus the standard applications of the black pencil, are all that I can claim to have done.

The reader will find therefore nearly as many kinds of composition as he might in an undiscriminating anthology; he will find exciting anecdotes and prosaic recitals; he may detect contradictions which merely display differences of opinion as to the fact, the deduction, or the emphasis; he will surely note exacting criticism of our Armed Forces close to the blandest of praise.

In one view this may be the best way to produce contemporary history. At least it does not interpose the present interpretation of all the events by a single protagonist, between the events and the later scholar, who, knowing none of the actors, may be able to view their antics with greater dispassion.

To be sure each of the contributors was himself a protagonist, or too close to a protagonist to be immune from suspicion as a special pleader. But in any event he speaks only for his own case and as a rule briefly. The reader may think he is interested in but one part of this book; but if he will persist and read it all he will begin to get an appreciation of the sort of zoo in which Dr. Bush was head keeper. He will be able to form his own judgments and not have to listen to the editor in the role of barker.

So what started as an apology has developed into a justification. It remains only to defend the individual authors against a criticism which might be alleged against them, that they have been too chary of dates in what is in a sense documented history. The reader will need to understand that the

omission of these dates has not been through the deliberate oversight of the authors but because of a requirement imposed upon them by those parts of the United States Government which are responsible for the handling of government interest in patent cases.

JOHN E. BURCHARD

CAMBRIDGE, MASSACHUSETTS

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# PART ONE

# Rocket Ordnance

## Development of Solid-Fuel Recoilless Weapons and Related Devices

*The History of Division 3, NDRC*

# *ACKNOWLEDGMENT TO PART ONE*

The full official record of NDRC consists of (1) historical narrative, and (2) technical reports and monographs. In Division 3, these two parts of the record were prepared concurrently, with the result that a great deal of the summarizing and evaluation of the technical contributions by the participating scientists had not been prepared in time for use in preparation of the history. This account, therefore, should not be regarded as a final authority on technical details.

In fact it is not meant to be such an authority. It is rather an attempt to tell, in a general way, the experience of civilian scientists working in conjunction with military experts on problems of developing weapons and devices for war — what those problems were and how one or another came to the fore as the pattern of war changed and gave rise to new necessities; how the problems were met and how the work of laboratories and testing ranges was translated into weapons for the fighting front; and finally, how those weapons went into Service use. For a detailed record of the scientific information accumulated in the course of this work, the technical reports and monographs are the final authority.

The authors, while in frequent consultation, have divided their responsibility instead of carrying on a continuous collaboration.

The sections on Orientation, Getting Under Way, OSRD Reorganization for Rocketry, Further History of the Eastern Group, and Termination, written for the most part by B. M. Norton, are based on a detailed record of Section H activities, written by the Section Chief, C. N. Hickman; on accounts by Professor William Huse of California Institute of Technology and Dr. Bryce Crawford of the University of Minnesota, prepared as the Division 3 contribution of basic information to the Joint (Army-Navy-OSRD) Board on Scientific Information Policy (JBSIP) for its pamphlet *U.S. Rocket Ordnance, Development and Use in World War II,* issued March 31, 1946; and on the JBSIP report itself. Contributions by R. E. Gibson, D. W. Osborne, S. B. Golden, R. B. Kershner, W. H. Avery, B. Weissman, D. A. Boyd, Marian Koritz, and Lieutenant V. Russillo have been used extensively.

Material provided by Dr. Clark Millikan, of the Guggenheim Aeronautics Laboratory (GALCIT), was used in writing part of Chapter XV.

For the section on the history of the work done under Division 3 at the California Institute of Technology, written for the most part by William Huse, many sources of information have been used. Chief among these were the many reports, bulletins, monographs, catalogues, and other publications issued by the Editorial Section of the California Institute group.

Scarcely less helpful was the information received from many members of the Research and Development and Service Sections, who responded patiently to barrages of questions and supplied additional material from their own experiences and from their files.

Acknowledgment is also gratefully made for information and assistance supplied by the officers of many bureaus and commands of the Navy Department. Much useful material was also derived from the following Navy Department periodicals: *Forward-Firing Aircraft Rocket Monthly Newsletter, Naval Aviation Confidential Bulletin, Naval Aviation News,* and *Rocket.* For information about the use of rockets by the 2nd Engineer Special Brigade, three articles by the Commanding General, published in the *Military Engineer,* were drawn upon, as was also the recently published Brigade history. Grateful acknowledgment is also made for additional information supplied by officers of the Brigade Headquarters, and of the Headquarters, 532nd Boat and Shore Regiment.

A debt of gratitude for advice and information is also due to Dr. R. C. Tolman, Vice-Chairman of NDRC; to F. L. Hovde, E. B. Bradford, P. T. Kirwan of Division 3; and to James Phinney Baxter, OSRD historian, and his assistant, G. R. Clark.

C. N. HICKMAN
W. HUSE
B. M. NORTON

Washington, D.C.
July, 1946

# Orientation

## CHAPTER I

## *INTRODUCTORY*

THE ROCKETS of World War II represented, not the invention of a new weapon, but the modernization of a very old one. Every major participant in World War II used rockets. Each nation developed rocket weapons in answer to its tactical and combat needs. The Russians pioneered in the firing of antitank rockets from planes and in the use of massed banks of rockets for pre-assault barrages. The British used rockets for defense against attack from the air at a time when there were only 500 antiaircraft guns in the United Kingdom. With the Luftwaffe driven from the skies, the Germans employed the long-range V-2 weapon to attack England's cities. The Japanese attempted to use rocket artillery to defend their island outposts.

The United States, too, developed and introduced a great variety of rocket weapons. By the time the war ended, American soldiers, sailors, and marines had fired millions of rockets at the enemy. For those rockets, our Armed Forces could thank the teamwork of American science, industry, and the military at home.

On Pearl Harbor day, our Army and Navy had not a single rocket in Service use. Plans for rockets were limited. By V–J Day, the Army was procuring rockets at the rate of $150,000,000 a year. The Navy had 1200 war plants in a program for turning out rockets at eight times this rate.

The arsenal of American rocket projectiles was led by Tiny Tim, a 1200-pound aircraft rocket which gave a fighter plane a heavier punch than a 12-inch gun. There were 5-inch and 4.5-inch aircraft rockets. For ground fire there was the powerful super-bazooka, and for amphibious assaults, 5-inch spin-stabilized rockets fired at a rate of 300 rounds per minute from the Navy's new rocket gunboats, equipped with automatic launchers.

One of the most spectacular rocket weapons of the war was the German V-2. Allied technical intelligence provided information about it more than a year before it finally hurtled out of the stratosphere to strike London. To counter, about all we could do was to attack the launching sites with every means at our command.

The Germans had been working on such a rocket since 1935. We had not. To start from scratch in 1943 to develop a similar American weapon would have required several years of intense research effort on the part of our scientists in order to provide the fundamental design data which German workers had accumulated over many years. The importance of such a weapon to the Allies was not great enough to warrant the diversion of scientific manpower already thinly spread over a vast military research program.

American rocket workers, both military and civilian, had to go at their job "cold." They had no store of basic data such as an adequate program of peacetime preparedness would have given them. Had it not been for the fact that the British had been more foresighted than we, had done experimental work with rockets before the war, and shared with us their knowledge and experience, the task of providing our forces with rockets would have been even more difficult and time-consuming. As it was, the job was hard and long enough, with every new project and improvement requiring an initial phase for the development of fundamental facts which a more complete program of peacetime military research would have developed in advance.

It is a tribute to the determination of the Army and Navy, to the imagination of American scientists, and to the productivity of American industry that, starting under such a handicap, they were able to create the rockets which hunted down enemy submarines in the Atlantic and Pacific, the bazooka, which was first turned loose against the enemy in North Africa, the aircraft rockets which spearheaded the Normandy break-through in July 1944, and the barrage rockets which laid down a deadly fire ahead of our forces landing in North Africa, Sicily, Italy, Normandy and Southern France, and on the beaches of the Pacific.

The program which produced such results was possible only because of continuous and close co-operation between the Army, the Navy, and the National Defense Research Committee of the Office of Scientific Research and Development.

Under NDRC a group of civilian scientists jointly with representatives of the Army and Navy began American World War II rocket development near Washington at Dahlgren, Virginia, and soon afterwards at Indian Head, Maryland. As the size and scope of the program grew the Indian Head activities were finally moved to the Allegany Ballistics Laboratory at Pinto, near Cumberland, West Virginia. A second group, with key personnel recruited mainly from the staff of the California Institute of Technology, carried on rocket-research and -development projects in Pasadena and on a number of military reservations in southern California and elsewhere, with headquarters on the Institute campus.

The Army and Navy participated more and more as rockets came out of the laboratories on their way to war. Many sections of the War and Navy Departments took on functions connected with rockets.[1]

But the story to be told here is that of the groups working on OSRD contracts under the supervision of what was designated officially toward the end of the war as the Rocket Ordnance Division of the NDRC, Division 3.

In the eastern part of the country, the division at the end of the war had its principal contract with the George Washington University,[2] which operated the Allegany Ballistics Laboratory, working primarily with the Army.

On the West Coast, the division had only one primary contract, with California Institute of Technology, but the California Institute used in all over 400 subcontractors in fabrication and supply of rocket components for the project, which was for the most part on behalf of the Navy.

The Chinese launched rockets against the Mongols at least as early as 1232 A.D. By the close of the thirteenth century, knowledge about rockets had traveled from the Orient to Europe, and a hundred years later was fairly widespread. Rockets were used to set fire to buildings and to terrorize the enemy. But as cannon were developed, rockets declined in warfare, and for many years after 1500 A.D. were employed only for signaling or for fireworks displays.

Crude rockets reappeared as a weapon toward the close of the eighteenth century in India, where native troops used them against the British. The British quickly undertook to develop military rockets of their own. They met with indifferent success until, in 1801, Sir William Congreve became interested in the problem. By 1805 he had rockets ready for Service use. During the Napoleonic Wars, British warships launched rockets against Boulogne and Copenhagen, and they were also used with effect at the siege of Danzig and the battle of Leipzig in 1813.

During the war of 1812, at the battle of Bladensburg, the raw American

[1]Among the Army field establishments which participated prominently in the problems involved in providing safe and effective rockets to our forces were Picatinny Arsenal, Aberdeen Proving Ground, Wright Field, and Dover Air Base, the last with a branch at Muroc, California. Naval establishments active on similar problems included the Naval Proving Ground at Dahlgren, Virginia, the Naval Air Station at Patuxent, Maryland, the Naval Ordnance Test Station at Inyokern, California, and the U.S. Marines Rocket Battalion at Camp Pendleton, California.

[2]Supporting contracts were also in effect with U. of Minnesota, Bell Telephone Laboratories, and the Budd Wheel Company. Earlier, there were also contracts with Hercules Powder Company, Budd Induction Heating, Catalyst Research Corporation, U. of Wisconsin, and Duke U. When developments reached a point suitable for service standardization, the Services in taking over frequently continued using contractors experienced under the NDRC program.

militiamen defending Washington broke under the flanking fire of British rockets, leaving the way open for the invaders to enter and burn the national capital. Three weeks later, the British Fleet attacked Baltimore. The story of Francis Scott Key needs no retelling — his detention on a British vessel in the harbor, his anxious night spent watching the bombardment of Fort McHenry and its "Star-Spangled Banner," his immortal expression of pride that the attack failed. What is worth pointing out is that the "rockets' red glare" of the national anthem is not a phrase born of a poet's fancy. For the British Fleet included several vessels equipped for rocket-firing. The rockets and their red glare were the real thing.

Though the Congreve rockets were erratic missiles, their author was a zealous believer in their future. Others shared his faith. Rocket units sprang up in a number of European armies. In the United States, too, a rocket battery was created in 1846 and ten more were organized the following year.

In 1846 an American, William Hale, made a significant improvement in design by substituting for a stabilizing stick three curved vanes attached to the rear end of the rocket. The propelling gases in flowing past the vanes made the rocket rotate about its longitudinal axis. Thus, Hale produced the first spinning rocket.

Two thousand of the Hale rockets were made in 1847 and saw service in the Mexican War. They were used in a landing near Vera Cruz and in the storming of Chapultepec, but little further information about them is contained in the records.

Despite a promising start, rockets as weapons went out of use soon after 1850. With the propellant powder then available, their development had been carried about as far as possible. Improvements in artillery — rifled gun barrels which increased accuracy and mechanism to absorb recoil — established a standard of efficiency with which rockets could not compete, until World War II brought new conditions.

# CHAPTER II

# *ROCKET FUNDAMENTALS*

A ROCKET is a missile propelled by the high-speed, rearward expulsion of gases generated by the combustion of an internally carried fuel. It works in obedience to the natural law first recognized in the late seventeenth century by Sir Isaac Newton who propounded, as the third of his "Laws of Motion": "For every action, there is an equal and opposite reaction."

The action of the gases from the burning fuel, in pushing rearward out of the rocket, is matched by an equally forceful reaction which pushes the rocket in a forward direction opposite that of the flow of the gases.

It is this reaction force which causes a rocket to fly through the air. It is *not* the push of the escaping gases against the atmosphere. A rocket can operate in a vacuum. In fact, it flies faster the thinner the air, because there is less atmospheric resistance.

These principles hold equally well for the simple Fourth of July rocket and the German vengeance weapon, the V-2.

Rockets are a special branch of the jet-propelled family. The V-2 was a true rocket in that it carried within itself (in a separate tank) the oxygen required for the combustion of its liquid fuel. The V-1, like most jet aircraft, depends on the atmosphere for its oxygen; strictly speaking, it is not a rocket.

The American military rockets discussed in this book are all true rockets. They carry their own oxygen with them, though not, like the V-2, as a liquid in a tank. They are propelled by solid fuels — smokeless powders containing the necessary oxygen for combustion.

A principal advantage of the rocket is its lack of recoil. Anyone who has fired a gun knows that the burning of powder in the combustion chamber drives the bullet out of the barrel and that the reaction to this forward push against the projectile is a backward push against the gun — and the person or the gun mount holding it.

Compared to a rocket, the speed of combustion in a gun and the gas pressure developed are extremely high. Because of the recoil and the high pressure, guns and gun mounts must be strong — which means weight. A rocket is without recoil in the sense that no metal parts are pushed backward. One might say that a rocket reverses the recoil effect. It shoots gases backward at high velocity and is itself pushed forward by the recoil.

Lacking recoil, a rocket can be fired from a simple, light tube or trough which serves merely to point the rocket in the right direction when it starts its flight. As a consequence, it may be fired from structures which could not hold up the weight of a gun and its recoil-damping mechanism or withstand a gun's recoil force. The comparatively light weight of rocket launchers gives the rocket a mobility enabling it to go many places impossible to a gun.

Although rocket launchers are usually lighter than guns, the rockets themselves, because of their motor weights, are normally heavier than shells and bombs of equivalent size and effectiveness. For repeated firing from fixed locations, this weight differential may complicate the problem of ammunition transport.

The blast of high-temperature high-velocity gases from the rocket is a hazard and an inconvenience. Launchers must be so mounted as to leave a clear space behind them, or blast shields must be provided. Also, the flash of rockets may reveal firing positions.

Many rockets have lower velocity than equivalent gun projectiles and hence are less effective in the penetration of hard targets.

Although aircraft rockets fired from aircraft in their direction of flight have accuracy comparable to that of guns, most ground-fired rockets do not; a large number of rockets must usually be fired to score a hit on a point target. This lower accuracy in ground fire, however, is of little consequence when it is desired to saturate an area target.

Basically all of the rockets dealt with in this book consist of a head or pay load, and a motor containing the propellant powder. Like shells and bombs, rocket heads may include solid shot, high explosive, smoke, incendiary, chemical, or other agents. To produce the effects desired at the target, nose fuzes and sometimes base fuzes are used; these are similar in function to those used in bombs and shells, but, because of the characteristics of rockets, are frequently different in construction and operation.

A glance at an assortment of standard American rockets will show that their weights range from 3 to nearly 1300 pounds, their diameters from 2 to 12 inches, and their lengths from 1 to 10 feet. In many rockets, most of the length is in the motor. In some, the motor is of smaller diameter than the head.

Some rockets have radial or cylindrical fins mounted at the rear end. Functioning like the feathers on an arrow, these fins stabilize flight through the air. Each of these rockets has one or several nozzles at the rear, parallel to the projectile axis. Other rockets, shaped more like artillery shells, lack fins. These have multiple nozzles set at an angle in the back end to impart rotation in flight, so that they are stabilized by spin, like rifle projectiles.

Fin-stabilized rockets perform best if long and slim like the arrow; spin-stabilized ones perform best if short and fat like a top.

The velocities of American military rockets vary from 65 to 1500 feet per second and their maximum ranges in ground firing are from 40 yards to 10,000 yards.

All of these rockets, except those of the bazooka type, continue burning for distances many times longer than the launchers. Distances of burning vary from 3 feet to 1000 feet for different rockets; corresponding durations of burning vary from 0.010 to 2 seconds. After burning has ceased, the rockets continue in free flight in the same manner as shells, and with similar trajectories.

For the powder used to propel rockets three definitions are needed:

*Smokeless powder* such as is used in rockets is a plastic material something like celluloid, composed of nitrocellulose and nitroglycerin (called "double base"), plus other ingredients to promote good burning. A *grain* of smokeless powder is a mass or body of powder of any shape and weight. Even though it weighs more than a hundred pounds, as some rocket charges do, it is still called a grain. Lastly, *the web of a powder grain* is the minimum cross-sectional thickness between two boundary surfaces; the web thickness controls the time required to burn the grain.

Burning[1] proceeds by parallel layers, preserving the general shape of the grain while reducing its thickness. The rate at which gases are generated by the burning of the propellant thus depends, for one thing, on the amount of exposed grain surface. To minimize the strength, thickness, and weight required in the motor wall to withstand the gas pressure, it is desirable that the pressure during burning remain uniform, without dangerous high peaks. Such behavior may be obtained by means of a constant burning area, which is achieved most simply by giving the grain the form of a tube. The exposed surface remains approximately constant, since, as the burning progresses, the exterior surface area of the tube decreases and the interior area increases. Another much-used cross-sectional form is that of a thick-armed cross, with portions of the arm surface "inhibited" to prevent burning.[2]

Just as rate of burning affects the pressure, so pressure affects the rate of burning. The higher the pressure, the faster the powder burns. The rate of gas production by the propellant and the rate of gas ejection through the nozzle must be in balance[3] at a pressure below the strength limit of the motor.

[1]The rate of burning is the rate at which one of the surfaces recedes, expressed in inches per second.

[2]Among the additional methods recently explored is control of gas-generation rate by embedding strands of special powders with proper temperature and pressure characteristics in the double-base matrix.

[3]The ratio of burning area of propellant to throat area in the nozzle—the first a factor controlling gas generation, the second, gas discharge — is an important quantity in rocket-motor design as the equilibrium pressure at which the motor operates depends principally on it.

The effects of propellant temperature on rocket performance and safety are much more marked than is the case with guns. The higher its *initial*[4] (before ignition) temperature, the faster the propellant powder burns.

At temperatures below the service minimum, the rockets fail to ignite, or burn only intermittently (chuff). At successively higher temperatures up to the service maximum, though velocity and range vary only slightly, acceleration increases and accuracy improves. At temperatures substantially above the prescribed service maximum, a proportion of the rocket motors will blow up near the launchers, with unfortunate results for personnel in the vicinity. Rocket users rather understandably like to have temperature limits labeled on their ammunition.

The principal components of a rocket motor are four:

*The Propellant Charge* — One or several large powder grains generate high-pressure gases rapidly; this charge is contained in the *Motor Tube,* which must withstand the gas pressure; one or more *Nozzles* at the rear end of the tube control the pressure, direct the gas discharge in smooth rearward flow, and improve thrust efficiency; and a *Powder Trap or Grid* supports and retains the charge during burning.

The gases generated by the propellant in a rocket motor exert equal pressure in all directions. Since they can escape only toward the rear, there is an unbalanced force forward which provides about 70 per cent of the thrust. In passing through a properly designed nozzle the gases expand to a lower pressure, with increased velocity, and by pushing against the outwardly flaring portion of the nozzle, they provide the remainder of the total thrust.[5]

Rocket research has inquired into the behavior of rockets, into rocket materials, and rocket processes. In particular, the rocketeer has had to deal with relationships among the many variables, and with their limits.

Partly because the standard bombs and shells already available met so many of the requirements for rocket heads, and partly because other problems were more urgent, comparatively little work was done on heads during

[4]Rate of heat transfer from chamber is too slow to raise temperature of grain interior before burning is complete.

[5]The thrust imparted to a rocket when unit mass of powder gas is discharged per second is a quantity which is of great interest in rocket design and which depends mostly on the thermodynamics of the propellent gas. A more extensive treatment of the technology of rockets may be found in several sources, now published or about to be published.

(1) *Mathematical Theory of Rocket Flight* by Rosser, Newton, and Gross, of Section H, Division 3.
(2) *Interior Ballistics of Rockets* by Wimpress, of Section L, Division 3.
(3) *Exterior Ballistics of Fin-Stabilized and Spin-Stabilized Rockets* by Davis, Blitzer, and Follin, of Section L, Division 3.

Certain other reports as they are declassified by the Army and Navy will be available from the Library of Congress and the Office of Technical Services, Department of Commerce.

the war. The rocket designer's main job frequently was to provide a motor which, when coupled to a head of specified weight, would give the whole rocket the velocity needed to deliver it to the required range.

This, though it may sound so, was no simple task. Perhaps the clearest way to see what the designer was up against is to picture him trying to make improvements in a rocket already designed.

If the pay load is increased, velocity and range, and perhaps accuracy, will be lost unless a much bigger motor is provided. If the designer tries to lighten his motor to compensate for the increased head weight, he finds that it becomes less strong, and hence requires a reduction in the maximum temperature for Service use. He may be able to improve the situation by the use of stronger alloys in his motor, but here he runs into higher costs and, in wartime, shortages of critical materials.

If increased velocity for the same head is required, he thinks first of increasing the amount of propellant. But this will require a longer, heavier motor. If he tries to pack more propellant into the same motor, he may find that he has choked off the gas flow; this increases the operating pressure and reduces the upper temperature limit. He may be able to use a thicker-web powder, if it can be made available, with less surface and longer duration of burning, to get more propellant in without choking the gas flow.

If his rocket is fin-stabilized and ground-fired, this may result in less accuracy. He may try to use a smaller nozzle to shorten the burning time and hence increase accuracy — this increases the pressure and lowers the safe maximum temperature. For other reasons he may want to use a larger nozzle; he finds more ignition trouble when the rocket is cold. For certain applications he can solve some of his troubles by using a propellant of different composition.

During the war, and especially in the earlier phases, rocket designers had even less freedom of action. Research had extended the limits of the design possibilities, but these extensions could not be put into practice until new plants were built to produce them. This was especially true in the production of thick-web powder grains — a production bottleneck almost to the end of the war.

# CHAPTER III

## *ROCKET HISTORY 1918–1939*

WHEN the United States entered World War I, Dr. Robert H. Goddard, Head of the Physics Department at Clark University, was engaged, under a grant from the Smithsonian Institution, in research on a rocket designed to attain high altitudes for meteorological purposes.

The rocket used the multiple-charge principle with the combustion chamber constructed so that successive charges of powder could be fed in through a breechblock mechanism operated by the "setback" forces.

Believing that rockets of this type might be useful in the war, officials of the Smithsonian obtained Government funds to expand this research, which was carried on under great secrecy in an isolated building at the Worcester Polytechnical Institute.

In the spring of 1918, the work was transferred to the Mount Wilson Observatory Shops in Pasadena, and H. S. Parker and C. N. Hickman, graduate students at Clark University, accompanied Dr. Goddard to California.

Since the complicated breechblock, so far, had not functioned, powder charges were fed through the nozzle instead, and in less than a week a model had been made and successfully fired in a cellar with the rocket attached to a ballistic pendulum.

This first model, which is described here as an example of the state of rocket development at that time, functioned as follows:

Charges were 2½-inch-long cylinders of 1/16-inch-wall cordite powder wrapped around #8 blasting cap with a molded percussive cap in the end. The burning gas discharge pushed a piston down a tube in the rear of the nozzle, thus releasing a spring to push another charge into position from a magazine in the fins. When the charge in the combustion chamber was burned, the reduction in pressure permitted a spring, acting through the piston, to hurl the new charge into the reaction chamber, after which the cycle was repeated.

This first model functioned satisfactorily on the ballistic pendulum but the high acceleration when it was tested in flight caused the rocket to pull loose from the rear mechanism. Although further tests showed the model could be made to work, the complexities in a satisfactory design for a multiple-charge rocket were such that the rocket pioneers decided that work

on a recoilless rocket gun using a single charge would be more likely to provide soon a weapon that could be handled by a single soldier.

The first rocket of this type, which had a 1-inch diameter and a length of about 18 inches, was propelled by a solid ½-inch-diameter grain of nitroglycerin powder. A high pressure was produced in the rocket motor tube, and the burning was over before the rocket left the 4½-foot launching tube. Flight tests demonstrated that the ballistic coefficient was higher than expected. Indeed in the first flight test the projectile went so fast that the workers were unable to see it in flight or to see where it fell.

In November 1918, Goddard demonstrated 1-, 2-, and 3-inch single-charge rockets, and a multiple-charge rocket at Aberdeen Proving Ground. In these tests Hickman showed the absence of recoil by firing rocket guns from his hands with shells weighing as much as 17 pounds. A 1-inch rocket gun was fired while resting in rings of two music stands.

Air Service observers were much interested in the larger projectiles, especially in a 4-inch model for aircraft weighing about 18 pounds, to have a velocity of 1000 feet per second. Then the Armistice was signed and interest in rockets dropped.

During the Twenties and Thirties, a number of enthusiasts in various countries devoted much time to the development of rockets. Probably the greatest amount of work was done by the German Rocket Society organized in 1927. Rocket societies were also organized in many other countries, including Austria, Russia, England; the American Rocket Society was founded in 1930. They were interested in rockets using liquid fuels — forerunners of the German V-2 and other long-range rockets. Liquid-fuel rockets are an excellent choice for such long-range applications, since such fuels provide higher velocities, lower, longer-sustained thrusts, reduced dead weights, and greater efficiencies. However, liquid fuels are difficult to handle, and so, for the usual short-range military applications, the solid-fuel rocket is the practical one. The work of these rocket enthusiasts during the Twenties and Thirties kept the rocket idea alive.

Between 1918 and 1940, Goddard continued rocket work with small grants he was able to obtain from various sources. In 1921, he did some work for the Navy on a rocket motor for accelerating bombs, but specifications demanded use of single-base nitrocellulose or black powder, and this prohibited progress. Not getting a satisfactory design of a multiple-charge rocket using solid propellant, Goddard turned to experiments with gasoline and liquid oxygen, and made a number of rockets which functioned properly except for a mechanism devised to provide safe return to the ground. This work was in progress in New Mexico with Guggenheim Foundation assistance when World War II began.

In June 1932, Lieutenant L. A. Skinner of the Army, who, after reading

the 1918 Aberdeen reports, became a rocket enthusiast, began tests on his own initiative and time, with limited funds and facilities. His experiments[1] were made with both solid and liquid propellants, with the main investigation involving accelerating a shell by a rocket after it had been fired from a trench mortar. He noted early that a suitable propellant was still to be worked out.

[1]Among his findings reported in 1936 were:

1. Gasoline and oxygen can be made to burn satisfactorily as a rocket fuel without use of an auxiliary tank of compressed nitrogen or a pump to force fuel into the combustion chamber.

2. The apparatus required is too complicated for rocket projectiles unless they are large ones.

3. For rocket projectiles, a solid fuel with proper burning characteristics and as much energy as possible is more practical of application than a liquid-fuel and gaseous oxidizing agent. In another report (1938) he pointed out it was desirable to have a powder which would give a "long flat curve", pressure of about 2000 pounds per square inch during most of burning.

# Getting Under Way

## CHAPTER IV

## *FORMATION OF SECTION H*

IN JUNE 1940, Hickman, now with the Bell Telephone Laboratories, and foreseeing the impending conflict, wrote Goddard asking his permission to present the possibilities of his rocket to appropriate Government officials, since improved materials and modern methods of fabrication might have much augmented the initial possibilities. An immediate reply was rereceived, "Go ahead, and God bless you!" Goddard reported, moreover, that he had himself made efforts to interest the Armed Services, but had made no progress.

Hickman then, with the approval of Mr. Guggenheim, presented the matter to Dr. F. B. Jewett, President of Bell Telephone Laboratories, and also Chairman of one of the divisions of the newly formed National Defense Research Committee (NDRC). Dr. Jewett was much impressed and asked Dr. Hickman to write up the story of the 1918 work and the possibilities of developing useful rocket weapons.

This story, submitted June 20, 1940, described the 1918 events previously noted, and continued with the following prophetic proposals for applications of the single-charge rocket:

1. Bombs which are dropped from airplanes might be made in the form of a rocket so that the velocity of the bomb as it drops would be materially increased, thereby increasing the accuracy or the elevation at which the plane could drop the bomb for the same accuracy as is now obtained at lower altitudes. If the tube in which the bomb is placed is so attached to the plane that it parallels the motion of the plane, then when the pilot dives toward the target and by means of sighting equipment, determines that this is the case, the rocket may be released with such a high velocity that its trajectory, for all practical purposes, is a straight line toward the target. It is a well-known fact that the velocity of the plane in the direction toward the target accomplishes a great deal in this respect. Now by adding to the velocity of the plane another 600 to 1000 feet per second, you will have a bomb dropping in a relatively straight line and with an impact velocity that may be high enough to penetrate many present day so-called bomb-proof structures.

2. The rockets may be fired from tanks. Since there is no recoil, the gun from which the rockets are fired will weigh a negligible amount and thereby make it

possible to mount guns on the tanks having large diameters and firing projectiles having very high destructive properties.

3. The rockets may be fired from such light guns that the infantry could be provided with projectiles which might have sufficient velocity to penetrate the armor of present day tanks. If this velocity is not high enough they might load the projectile with a combination of thermite and some other material which will adhere to the tank and thereby destroy it even though penetration is not achieved.

4. Submarines might be provided with these guns for use in firing at destroyers which may be attempting to destroy the submarine and which might be able to do so before the submarine can orient itself into proper position for using its torpedoes. A number of these guns could be used and fired independently in any direction and thereby have a better advantage when engaged by more than one destroyer.

5. Very small boats would be capable of carrying relatively huge bombs and shelling larger ships or coastal defenses.

Jewett forwarded copies of this report, together with other documents discussing rocket principles, estimated data for projectiles of approved design, and specifications for a 4-inch recoilless rocket gun applied to battle planes, to Major General C. M. Wesson, Chief of Ordnance, to Admiral H. G. Bowen, Director, Naval Research Laboratory, and to Dr. R. C. Tolman, Chairman, Division A, NDRC (Armor and Ordnance), suggesting that he was personally much impressed and that General Wesson and Admiral Bowen would perhaps wish to confer with Dr. Hickman. The only interest shown by the Services in these proposals was by the Bureau of Ordnance, U.S. Navy, in the one to accelerate bombs.

The skepticism regarding the military usefulness of rockets is not surprising, for the rockets actually developed in 1918 had had a rather short range (700 yards) by modern standards and a very low velocity; moreover, they were inaccurate as compared with a gun. It took a scientist, with experience in the field of rocketry, who realized how relatively crude these early rockets were and how much they could be improved, to grasp their potentialities.

Hickman was then invited to Washington to be interviewed by Tolman, who reported the meeting to Dr. Vannevar Bush, Chairman of NDRC. Shortly after this, July 23, 1940, Dr. Bush wrote Hickman, appointing him Chairman of a section in Tolman's NDRC Division of Armor and Ordnance.

# CHAPTER V

## *INITIAL WORK AND TESTS 1940–1941*

THE ESTABLISHMENT of Section H[1] in Division A marks the formal action initiating research and development in rocket ordnance under NDRC.

Hickman began his work with extended conferences with Captain G. L. Schuyler, Commander G. C. Hoover, and G. A. Chadwick of the Bureau of Ordnance, U.S.N., concerning the problem of accelerating the 14-inch AP (armor-piercing) bomb then under development. It was agreed that initial experiments should be made with some 6-inch models available from armor-penetration tests. Dr. L. T. E. Thompson, Physicist, and Commander W. S. Parsons, Experimental Officer at the Naval Proving Ground, were especially instrumental in getting the work started.

Of course, laboratory and range facilities were necessary for the experiments, especially in view of the hazardous nature of work dealing with rockets. The Navy offered a small corner of the Naval Proving Ground at Dahlgren, Virginia. It was here that the first rocket experiments were made, with the first shot fired in September 1940.

In the autumn of 1940, a British scientific mission, headed by Sir Henry Tizard, came to the United States. This group had requested and procured the permission of the War Cabinet to provide American scientists with data concerning many of the principal items in the highly secret war-research program of the British. The purpose of this mission was to enlist the help of the American laboratories on many long-range research problems for which the British had neither the technical manpower nor the requisite facilities. Here was the beginning of the close co-operation and the free interchange of ideas between the rocket scientists of the United States and those of Great Britain. The value of receiving the information offered by the British was immeasurable — particularly in view of the fact that they had gone to work in their laboratories several years earlier, laying the groundwork for the development of many new weapons which were put to use in this war. Because of British co-operation the men in the American laboratories were able to catch up quickly and to eliminate months of trial and experiment which would otherwise have been essential.

[1]This section was the second organized. It became known as Section H in accordance with a practice of designating a section by the initial letter of its Chairman's surname, in this case Hickman.

The first consultant[2] appointed to Section H was E. Lakatos, a Bell Telephone Laboratory engineer. He was the author of the first Section H report on the internal ballistics of rockets. Captain Skinner was appointed by the Ordnance Department as a special liaison officer.

In the beginning, NDRC transferred funds to the Navy, which supervised certain operating expenditures. Also, mechanical work was carried on by means of a contract with the Department of Terrestrial Magnetism of the Carnegie Institution of Washington, under Section T of Division A. Both this organization and later Section E at the National Bureau of Standards designed and constructed equipment for Section H.

Double-base powders (nitroglycerin and nitrocellulose) were the only available propellants with enough energy for efficient use in rockets. The Hercules Powder Company was the only concern making the powders of compositions suitable for the rocket tests, and of these a double-base powder, ballistite, which was similar to British cordite, seemed to be the most promising. But ballistite was produced here in only two forms — in sheets for trench mortars or in small, thin-web grains for small arms.

There are two methods of extruding double-base propellant: solvent and solventless.[3] In the former, the powder, suspended in a solvent which swells the nitrocellulose (permitting colloiding with a small amount of mechanical work), is made into a dough and then is extruded through dies; the sticks, or grains, that are formed "dry out" afterwards by evaporation of the solvent. Satisfactory drying is obtained only with relatively thin sections of powder with a web less than ½-inch. In the solventless method, used for large thick-webbed grains, the mix is colloided by hot-rolling into sheets; the sheet powder is rolled into "carpet rolls," or cut into chips, warmed slightly in air, and forced through a die by hydraulic pressure. The solvent process was the only one practiced in the United States; the British had long produced cordite grains by solventless extrusion.

Consultation with British representatives, Fowler and Cockcroft, and soon after with Whitworth of the British Purchasing Commission, formerly of Imperial Chemical Industries, resulted in attempts to arrange with the Hercules Powder Company for the dry extrusion of double-base powder which would be required if rockets giving the performance of the British antiaircraft rockets (reaching altitudes of over 20,000 feet) were to be developed.

The Hercules Company was reluctant on account of the hazards to undertake dry extrusion of American powder at its Kenvil factory, and suggested an alternative procedure for producing larger grains, involving cementing together disks of rolled sheet powder by heat and pressure.

[2]Dr. Goddard accepted appointment as a special consultant, although he was planning to work for the Navy on liquid-fuel rockets instead of the powder type used by Section H.

[3]Also referred to as wet and dry.

American ballistite had higher energy and hence was more hazardous than the British cordite.

At this time, a problem of great concern was trapping the powder in the rocket motor so that unburned portions of the grains would not be ejected by high-pressure gases in front of the ends.

Methods for preventing this that were tried, but unsuccessfully, included (1) plugging the lengthwise central perforation of the grain at the front end and painting with shellac, (2) embedding the front end in plaster of Paris, and (3) having the nozzle extend into the combustion chamber beyond the rear ends of the powder sticks.

A metal cylindrical, multiperforated powder trap, tried later, was more satisfactory and this was improved on by doing away with the outside walls next to the perforations, forming slots, and in this way preventing an undesirable pressure differential from building up between inner and outer walls of the metal cylinder during burning.

Tests carried out at Dahlgren, in which this method of trapping was used, showed that 6-inch model bombs could be accelerated to a velocity of 210 feet per second, which was as much as had been requested.

Further improvements were made later in trap design. Major Skinner devised a hairpin model which Hickman improved by having the ends terminate in a so-called "wormhead." The final design consisted of a stamped ring with scallops through which the cage wires passed. The centrally perforated grains of solvent-extruded powder were hung on these cage wires. The wires themselves had rivet heads for the purpose of retaining the scalloped ring.

The layman reader in pursuing an account of work of this kind should be warned against getting the impression that, because the story emphasizes and reports mainly the successes of the experimental programs, the workers need only be given a problem to go to their desks and laboratories and produce solutions in quick order. No scientist reader needs to be told of the grief normally a part of experimental work.

This bothersome experience in trapping is only an example.

As increasing demands on the use of the facilities at Dahlgren made it difficult to schedule the rocket tests, a move to the grounds of the Naval Powder Factory at Indian Head, Maryland, was made at the suggestion of Dr. Thompson. Here there was an old Navy Proof Valley used only for storage.

The Indian Head work was guided by a group called the Jet Propulsion Research Committee. The original members were Commander Hoover, Dr. C. C. Lauritsen,[4] Dr. Thompson, and Dr. Hickman.

Lieutenant H. E. Baker was assigned by the Navy to take charge of a Service unit for the rocket program; but for a time Hickman, Skinner, and

[4]Professor of Physics, Cal. Tech.

Baker alone loaded the rockets, fired them, recorded the data, and did the janitor work at the "Bomb Proof" shelter. Machine work was done by Captain Skinner at home, and Hickman manufactured squibs and made black-powder primers, using bags made from silk stockings. This was the scale of American rocketry in 1940!

Later in that year, drop tests were made with the 6-inch scale models of the 14-inch AP Bomb. They were highly successful. The tests were made at dusk, and as the first bomb dropped, all eyes beheld a spectacular event. As the bomb neared the target it suddenly darted forward with an increase in velocity that appeared very great, the luminous jet darting rearward in the fading light.

# CHAPTER VI

## *THE LAURITSEN REPORT*

IN THE first half of 1941 the rocket test program was concerned with the jet-accelerated AP Bomb and aircraft-rocket problems. These produced design and test[1] techniques, which would be applied to later projects.

Meanwhile attempts to obtain large grains by cementing sheet-powder washers together by heat and pressure were unsuccessful, nor did coating the washers with dibutyl phthalate before applying the heat and pressure work. After several experiments the conclusion was reached that these methods involving sheet powder were not sufficiently dependable for thick-webbed grains.

In June, ¼-inch and ¾-inch grains were dry-extruded in the Bomb Proof at Indian Head, in a small homemade extrusion cylinder with an arbor press procured from the machine shop, but experiments were discontinued with these facilities on Navy orders, due to the hazards.

On July 30, 1941, a conference of the Jet Propulsion Research Committee was held at the Bureau of Ordnance. E. C. Watson,[2] Acting Chairman of Division A, and R. E. Gibson, physical chemist from the Geophysical Laboratory of the Carnegie Institution of Washington, a consultant of Section H, were also present.

Here it was reported that Hercules was prepared to construct presses for the dry extrusion of double-base powder at the new Radford Plant, and some drawings of British facilities were made available for this purpose. At this same meeting some encouraging success with the cementing of propellant with dibutyl phthalate was reported, and Hercules was also instructed to continue work along this line.

The expansion at Radford would be under direct Army Ordnance Department contract. Meanwhile samples of solventless extruded ballistite were to be extruded and tested by Section H to determine if it was satisfactory for use in rockets.

As the Indian Head Jet Propulsion Committee was concerned only with the work at that facility, a recommendation was made that NDRC draft a

[1]Illustrative of those developed then and later are:

(1) Improved pressure-time recording instrumentation for study of internal ballistics
(2) Partial burning apparatus (known as "Jumping Jack")
(3) Special ovens and refrigerators for use in rocket ranges.

[2]Professor of Physics, Cal. Tech. — now Dean of the Faculty.

letter to the Secretaries of both War and Navy, proposing that a policy committee of some sort be set up to handle recommendations for new developments.

In this connection, the following memorandum prepared by C. C. Lauritsen, Vice-Chairman of Division A, who had recently returned from an extended visit to England in behalf of interests of the division, was sent to Dr. Bush. It is quoted in full here because it gives an authoritative picture of the status of rocket development at that time and was instrumental in leading to NDRC's decision to expand its rocket activities.

Memorandum: 1 August 1941

To: Dr. Vannevar Bush

From: C. C. Lauritsen

Subject: Expansion of Program of Rocket Developments

It has been my opinion for some time that there is an urgent need for a considerable expansion of the whole program of development of rockets for military purposes now under way in this country. This opinion became a firm conviction as a result of my investigation, while in England this spring, of the rocket work being done there. A large part of my time while in England was devoted to this problem and I came back more than ever convinced of its importance and urgency.

Before making specific recommendations it may be well to outline briefly (1) the work that is under way in this country and (2) the work that has been done in England.

(1) Official government work on rockets in this country is, I believe, limited practically to three fairly distinct projects. These are:

(a) Assisted take-off and accelerated flight for airplanes.

The NACA has a Special Committee on Jet Propulsion of which Dr. Durand is Chairman and a high priority has been given to this work by both the Army and the Navy. Research and development work is under way at the Guggenheim Aeronautical Laboratory, California Institute of Technology, Pasadena, California, and at the Engineering Laboratories, U.S. Naval Academy, Annapolis, Maryland. The former work is financed by an Army Air Corps contract and the latter by the Navy Bureau of Aeronautics. In addition, the Navy Department is having conversations with Dr. R. H. Goddard, with a view to using him as consultant or to the authorization of a definite contract for the application of his methods and experience to the solution of this problem. Dr. Goddard has been working at Roswell, New Mexico, since 1930 under a grant from the Guggenheim Foundation of New York City.

(b) Rockets for plane-to-plane use.

Since the rocket action does not transmit recoil to the airplane that is doing the firing, it becomes possible to use large calibers.

(c) Armor-piercing bombs.

Jet propulsion can be supplied to accelerate armor-piercing bombs in order to secure penetration of the decks of enemy ships even when the bombs are dropped from relatively low altitudes.

The two projects (b) and (c) have been the concern of Section H, Division A, NDRC. Since the establishment of our Joint Committee on Jet Propulsion with the Bureau of Ordnance, U.S. Navy, much of the work has been done with the help of facilities set up at the Naval Powder Factory, Indian Head, Maryland. Liaison with the Army has been maintained through an officer actively engaged in this development. This committee has recently been expanded to include an officer designated by Army Ordnance. It now consists of: Lt. Commander J. A. Snackenberg, U.S. Navy, Bureau of Ordnance, chairman; Major L. A. Skinner, U.S. Army Ordnance; Dr. L. T. E. Thompson of the Naval Proving Ground, Dahlgren; Dr. C. C. Lauritsen, Vice-Chairman, Division A, NDRC; Dr. C. N. Hickman, Chairman, Section H, Division A, NDRC; Dr. J. E. Henderson, Consultant, Division A, NDRC.

The study of these two applications of rockets, using model and full-scale experimental rocket projectiles, has reached a stage of considerable success, but it is not yet at the point where final designs for production are warranted. Section H, Division A, NDRC, has also considered the possible development of high-speed rockets for use against enemy aircraft and of rocket devices for setting up aerial mines as protection against enemy bombers. Both of these developments have been temporarily dropped, however, because of the lack of proper facilities and of sufficient personnel at Indian Head. In fact the Indian Head group feels that it should concentrate upon the two aforementioned projects (b) and (c) to the exclusion of everything else until these problems have been solved. The knowledge gained from these two applications could then be applied to other rocket problems.

To obtain a satisfactory propellant powder for their work, the Indian Head group has had to resort to the use of double-base powder. Although the British employ a dry-extrusion process to manufacture their rocket powder, American manufacturers undertake extrusion only with the admixture of solvent. Unfortunately this American powder when made with the large web thicknesses required for most rocket applications retains too much solvent even after prolonged drying. At our instigation the Hercules Powder Company has begun work on the formation of sticks of cordite from the thin sheets of solventless powder now manufactured for use in trench mortars and is also considering the possibility of setting up, at Radford, experimental presses for duplicating the extrusion process used successfully by the British. Until this powder problem is successfully solved most of the rocket work under way in this country will be seriously curtailed.

(2) Although many millions of dollars have been expended in England upon the development and manufacture of rockets, to obtain a reliable evaluation of their importance as an antiaircraft weapon was exceedingly difficult. The proponents were optimistic, the most rabid of them claiming that rockets would soon replace antiaircraft guns; the critics complained that the results had not been commensurate with the efforts, that rockets would always be inaccurate weapons, although useful when guns are not available. However, all agreed that the rocket was an important development that must be continued at all cost.

Although the dispersion with antiaircraft rockets in their present stage of de-

velopment certainly is at least five times that with guns under ideal conditions, in actual firing against a moving target this factor probably is more nearly two, owing to the fact that the tracking and prediction errors are large in both cases. Specific reasons can be cited for thinking that the dispersion is mainly due to poor performance of the present propellant and would be decreased if the internal ballistics could be improved. For example, slivers often are observed long after the acceleration; these sometimes burn in the chamber but often are ejected and burn in free space for several seconds, thus indicating a considerable loss of propellant. The remedy may be either an improved method of supporting the propellant or the development of a more suitable propellant.

Facilities in England are entirely inadequate for the manufacture of the propellant used at present in AA rockets — a solventless cordite extruded in the form of long hollow cylinders. A new plant at Bishopton has reached only 10 per cent of its capacity, and the product is not as satisfactory as that from the older plants. Steps are being taken to improve the product but the results are still inconclusive. The best remedy obviously would be an improved propellant but this may be a long-time project. There is very keen interest among the ordnance authorities in England that some manufacture of cordite be started, for whatever reason, in America.

The following types of rockets are already in use in England:

(a) The 2-inch rocket.

The 2-inch rocket — the first weapon to be developed — is intended mainly for use on shipboard against dive bombers and low planes. For firing this rocket several types of projectors have been developed; the favorite seems to be a turret-like structure with open rails for firing 20 rounds simultaneously. There is talk of increasing this to 40 rounds. No predictor is used with this projector; it is operated by one man who occupies the turret.

The 2-inch rocket is economical and easy to handle. Although it appears attractive and convenient — especially for small merchant ships which cannot carry big guns and predictors — there is no evidence that it has been of practical use as yet. However, the interest of the Navy in this weapon may be judged from the fact that it has just placed an order for 4,500,000 of this type alone.

(b) The 3-inch rocket.

The 3-inch antiaircraft rocket is intended for use against dive bombers and high-altitude planes. Its present ceiling is said to be 22,000 feet when used with a time-fuse, somewhat less with a photo-electric fuse and about 28,000 feet with a lighter head.

Although the projector now in service use is a light mobile single unit, a double unit is under development. The plan is to have as many as 64 projectors operated from a single predictor and fired simultaneously. At present, 100 3-inch antiaircraft guns are being modified to accommodate rails for firing 9 rounds of 3-inch rockets; these units are intended for mobile use. There are few developmental problems in England on which so much money and effort is being spent as on the 3-inch antiaircraft rocket. Although still in the developmental stage, this rocket is already being produced in large quantities and is extensively used. In connec-

tion with this development there is a separate proving ground employing 300 men. A special regiment of the coast artillery consisting of three batteries is also assigned to this work and serves to train crews.

Doubtless this rocket is already a very useful weapon but it would be much more useful if a better fuse were available. At present the only fuse in use is a powder train time-fuse which is initiated by the pressure in the nose. A photo-electric fuse is in production, but it is not yet entirely satisfactory. A radio proximity fuse is under development. The British antiaircraft command is much interested in this rocket and is most anxious to have the radio proximity fuse perfected. It also expressed the opinion that a continuously adjustable time-fuse would be a great improvement over the present pre-set fuse. The general in command is urging the development of faster and larger rockets with a ceiling of 40,000 feet. There is room for improvement in the ballistics, the mean deviation being about one degree, and there is every reason to believe that improvement can be made by relatively simple improvements in the propellant and its mounting in the rocket. There is also much room for improvement in the projectors, both single and multiple, as well as in the fire-control apparatus. At the present state of development it is estimated that two 3-inch rockets are about as effective as a 3.7-inch shell. With a continuously adjusted time-fuse the rocket would probably be slightly better than the shell and with a reliable proximity fuse it would be more than five times as effective.

(c) Wire barrage rockets.

One of two existing types of wire barrage rockets consists of a 2-inch rocket that pulls a wire and bomb out of a stationary container; it is intended for laying a barrage from 1000 to 2000 feet. The other type is for much higher altitudes and consists of a 3-inch rocket with a special head that carries the parachutes, wire and bomb.

These devices function well mechanically, more than 90 per cent of them opening properly and descending quite slowly. The claim is that such wire barrages have brought down or seriously damaged a number of German planes, and the Germans have equipped some planes with "wire cutters" and "fender wires" which are said to reduce the airplane speed by as much as 50 miles per hour.

(d) The 5-inch rocket.

The 5-inch rocket is intended to carry a 30-lb. bomb. It was designed originally for use with chemical bombs. Although the few that we have seen fired had square noses and tumbled badly, this defect doubtless can be corrected if desirable.

Work is in progress on an antisubmarine bomb that uses the same rocket. This bomb has a very blunt conical nose designed for proper entry into the water. It is intended for high-angle fire and is said to have good underwater ballistics.

This summarizes the rocket situation as it exists at the present time. What we desire to know at this time is whether the Armed Services think that further rocket work, and in particular work on the high-altitude antiaircraft rocket, should be initiated at this time. As previously pointed out, no work on AA rockets has been done in this country so far. This is largely due to the fact that no suitable propellant has been available. It now appears that such propellants would be avail-

able shortly, at least in sufficient quantities for developmental work. Plans are under way for the construction of two extrusion presses of the British type for the production of this material in quantities sufficient for training purposes, and other methods of forming large sticks of cordite, initiated at Indian Head, are being developed by the Hercules Powder Company. The development of high-altitude antiaircraft rockets can therefore be started at once, provided such a project is of sufficient interest to the Armed Services. However, it cannot be undertaken by the group at Indian Head without seriously interfering with the important work now in progress there. Furthermore, a considerably larger firing range will be required and the work should be carried out in the closest possible collaboration with the antiaircraft units of the Coast Artillery. An arrangement similar to the one we now have at Dahlgren should be made with the Coast Artillery. The latter should provide targets, gliders, drones, etc., and should conduct the firing trials. A range, such as the Coast Artillery Range at Barstow, California, should be designated; there it is possible to fly every day. We have gone to Dahlgren again and again without being able to carry out scheduled tests because of unfavorable weather conditions.

Many of the objections which are now advanced against rockets can probably be met by improvements in the design, not only of the rockets themselves, but also of the propellant, the projectors, and the fuses. Our photo-electric fuse is now quite satisfactory and can be placed in production on short notice if necessary. Its use, however, is limited to daylight and favorable flight conditions. Our radio proximity fuse is very promising and several tests have been successful. It is reasonably certain now that this fuse will be the most satisfactory when it becomes available. Work has also been started on an electric time-fuse which can be continuously adjusted while in the projector either by hand or by a predictor.

If the Armed Services do not give the high-altitude antiaircraft rocket a fairly high priority, then we should revaluate everything that is being done at the National Bureau of Standards on proximity fuses and much of the powder development we have under way can be postponed.

To be still more specific, we should like answers to the following questions:

1. Do the armed forces think that the rocket work already under way should be expanded?

2. What sort of priority will the armed forces give to the high-altitude antiaircraft rocket?

3. Do the Army and Navy look to us to keep them posted on rocket developments and to make recommendations regarding promising developments or do they prefer to make such investigations themselves and suggest projects to us? The Indian Head Committee feels that it has no authority to suggest new projects. If they do not have this authority, who does? If it is the function of NDRC to recommend new developments to the Army and Navy, then we should recommend:

(a) The development of a 2-inch rocket with contact fuse, similar to that of the British, followed by educational orders and experimental use by the Services.

(b) The development of a 3-inch rocket, similar to that of the British, but with urgently needed improvements, such as proximity fuses and continuously adjustable time-fuses.

(c) The development of a 4.5- or 5-inch rocket for altitudes up to 40,000 feet.

(d) The development of antisubmarine and chemical warfare rockets.

4. If the development of AA rockets is approved by the armed forces, should the projects be financed by NDRC or by the Coast Artillery?

5. Can the antiaircraft command of the Coast Artillery be authorized to co-operate with NDRC or its contractors in such a developmental program in the same way in which the Naval Proving Ground at Dahlgren now co-operates in the development of proximity fuses?

6. Should a rocket committee composed of representatives of the Army, Navy, and NDRC (with perhaps a British representative) be set up to determine policies, make recommendations of new projects, evaluate priorities, standardize manufacture, etc.? There is a definite need for a standardization of American and British practice in regard to sizes of rockets manufactured and of all associated apparatus such as fuses, projectors, and predictors.

C. C. LAURITSEN
*Vice-Chairman, Division A*

Shortly after this the Services indicated interest in rocket development for a wide variety of uses; for plane-to-plane fire; for low- and high-altitude antiaircraft fire; for use as targets; against submarines; to carry chemical warfare ingredients; to provide jet acceleration for armor-piercing bombs, and jet assistance for aircraft take-offs; for catapults; and for long-range bombardment.

The NDRC plans now required a great expansion of Section H activity. New members[3] were appointed to fill out the organization and they attended the first meeting on August 19. Within the next few weeks they recommended approval of contracts which then seemed large and now seem small, $25,000 for General Electric Company, $10,000 for Western Electric Company, $200,000 for the California Institute of Technology. In October a further contract was authorized with George Washington University.

Under these, Western Electric was to furnish engineering services and special material through the Bell Telephone Laboratories, which had been

| | |
|---|---|
| [3]Dr. E. U. Condon | Westinghouse Mfg. Co. |
| Dr. W. D. Coolidge | General Electric Co. |
| Dr. A. J. Dempster | U. of Chicago |
| Dr. G. B. Kistiakowsky | Chairman, Explosives Section of Division B, NDRC |
| Dr. Warren Weaver | Chairman, Fire Control Section of Division D, NDRC |

C. C. Lauritsen, Vice-Chairman Division A, and E. C. Watson, Acting-Chairman Division A, and C. N. Hickman were also present, and the Services were represented by Lieutenant Colonel H. W. Dix and Major L. A. Skinner, Ordnance Department, U.S.A. and Commander J. A. Snackenberg, Bureau of Ordnance, U.S.N.

making donations in kind for several months; General Electric was to develop a continuously adjustable time fuze; George Washington University was to supply materiel and personnel for the operations at Indian Head; California Institute was to start a new program of broad scope to develop and test rocket devices, to develop, adapt, and test associated equipment such as proximity fuzes, arming and safety devices, projectors, and to carry on other development work in ordnance as the Services might request. The last two of these contracts initiated the operations of the two contractors who were to spearhead all the work of what became Division 3: George Washington University on the Atlantic coast, California Institute on the Pacific.

# OSRD Reorganization for Rocketry

## CHAPTER VII

## *ORGANIZATION AND ADMINISTRATIVE HISTORY*

THE ESTABLISHING of central laboratory contracts (GWU and Cal. Tech.) marked a turning point in OSRD rocket development. With these it was possible for the first time to secure the talents of an adequate number of competent scientists and engineers, many of whom were recognized leaders in their peacetime fields.

The histories of the two groups centered on the East and West Coasts are so divergent as to require separate accounting rather than a single chronology. But before these two paths are traced it is necessary to digress with a few pages of administrative history in order that the reader may learn something of the governmental organization under whose auspices the work was accomplished.

By May 1942 the work at the California Institute had greatly expanded, and had become concerned mainly with the rocket propulsion of antisubmarine bombs. Therefore a new Section of Division A, designated Section C, was established under Dr. J. T. Tate[1] who was in charge of the NDRC antisubmarine program. The new Section C was set up for work on antisubmarine Ordnance. It assumed direction of the work at Cal. Tech. under Contract OEMsr-250, and studies on underwater properties of projectiles which had been initiated by Division C[2] Institute group under Contract OEMsr-329. Both of these contracts were superseded by a single contract, OEMsr-418, under which the whole Section C activity at Cal. Tech. was continued.

The division in the East, through Section H, had the task of administering and co-ordinating a number of prime contracts, while on the

[1]Professor of Physics, U. of Minnesota.

[2]Division C of NDRC had charge of the antisubmarine program. Later in reorganization one branch became Division 6.

West Coast there was only the one prime contract with various parts of its organization covering all the needed aspects of rocket activity.

Furthermore, although the Section H group began joint operations with the Navy at Dahlgren and Indian Head, as the war progressed, this group became closely affiliated with the Army rocket program. In this collaboration the Army took over at relatively early stages and itself, to a considerable degree, carried out standardization tests, made design changes, devised auxiliary equipment and handled production problems of the weapons developed.

On the other hand, the Cal. Tech. program became one which was carried on almost exclusively in close association with the Navy, which followed a policy keeping the NDRC scientists, who did the original designing, in effective control through the early production stages, and which provided also for their assistance in training programs.

In December 1942, NDRC was reorganized. Tate became Chief of Divisions 3 and 6. Division 3 was formed by the recombination of the two rocket Sections (C and H) of Division A, with Hickman, Gibson, Watson, Lauritsen, Lacey, Kistiakowsky, Ellett, and Hubble as members.[3] Division 3,[4] later named Rocket Ordnance, was at this time called Special Projectiles.

[3]W. N. Lacey is Professor of Chemical Engineering at Cal. Tech. G. B. Kistiakowsky, Professor of Physical Chemistry at Harvard U., was Chief of Division 8, which was developing rocket propellants of types other than double-base powder. A. Ellett, Professor of Physics, U. of Iowa, headed Division 4, then concerned mainly with proximity fuzes for rockets. E. P. Hubble, Astronomer at the Mt. Wilson Observatory and with the Ballistics Division at Aberdeen, represented the scientists working there on Army rocket developments.

[4]The staff of the division consisted of the following Technical Aides:

L. G. Straub, Executive Officer, Professor of Hydrodynamics, U. of Minnesota
E. B. Bradford, prior to the war a geophysical engineer
P. T. Kirwan, formerly a chemist with U.S. Customs Lab., Natl. Bureau of Standards
A. Kossiakoff, Instructor in Chemistry at Catholic U. of America.

John G. Roberts was Patent Counsel.

Consultants appointed immediately or soon after were:

H. B. Alexander, Hercules Powder Co.
John Beek, Jr., Bureau of Standards
Robert Burns, Bell Telephone Labs.
R. W. Cairns, Hercules Powder Co.
B. L. Crawford, U. of Minnesota
Farrington Daniels, U. of Wisconsin
R. H. Goddard, Clark U.
K. F. Herzfeld, Catholic U. of America
J. O. Hirschfelder, U. of Wisconsin
M. Hobbs, Duke U.
Emory Lakatos, Bell Telephone Labs.
W. B. McClean, Dept. of Terrestrial Magnetism
F. T. McClure, U. of Wisconsin
F. V. Malina, Cal. Tech.
B. B. Owen, Yale U.
J. W. Parsons, Cal. Tech.

In February 1943, F. L. Hovde,[5] Executive Assistant to Chairman, NDRC, was named Staff Aide to maintain liaison between the Chairman's Office and Division 3.

Later in the year NDRC was in doubt whether it should continue its rocket activities centering at Indian Head, since NDRC work there on Navy projects had become of minor proportions and since there were indications that the Army intended to concentrate all work on its rocket projects at its own establishments at Aberdeen, Picatinny, Radford, and elsewhere. After a number of conferences and exchange of correspondence between General Barnes and Dr. Conant, an NDRC decision not to recommend continuation of the work at Indian Head was reversed. A program in co-operation with the Ordnance Department was approved, the work to be under the supervision of a special committee with Colonel I. E. Luke as Chairman and Gibson, Hickman, and Hovde serving as NDRC representatives.

In June 1943, Dr. Tate resigned as Chief of Division 3 to devote his full time to the activities of Division 6. Until September, Dr. C. C. Lauritsen served as the temporary head of the Division 3 group. During this short period, Section H was responsible directly to the Chairman's Office. In September, the division was reorganized with two sections, H and L; Hovde was Chief and the members were Hickman, Gibson, C. C. Lauritsen, Watson, Tate, Kistiakowsky, and Ellett. Hickman was Chief and Gibson Deputy Chief, of Section H, and Lauritsen was recommended as Chief of Section L. A proposal for Section committees to assist the Section Chiefs was made. This was particularly desirable because the principal laboratories of the two sections were separated by the width of the continent, and it was therefore difficult for persons associated with activities on one coast to be in close touch with operations on the other. However, OSRD approval

L. C. Pauling, Cal. Tech.
J. B. Rosser, Cornell U.
M. M. Safford, General Electric Company
B. H. Sage, Cal. Tech.
C. P. Saylor, Bureau of Standards
H. F. Stimson, Bureau of Standards
L. S. Taylor, Bureau of Standards
J. R. Townsend, Bell Telephone Labs.
F. H. Untiedt, Patent Attorney, Washington, D.C.
B. D. Van Evera, GWU.
R. J. Walker, Cornell U.

A Special Divisional Committee under the chairmanship of Lacey, with members Gibson, Kistiakowsky, Sage, and Beek, was established to survey and co-ordinate propellant programs.

Under the new National Defense Research Committee procedure, the reviewing Subcommittee of that body established for Division 3 consisted of R. C. Tolman, Ch., Roger Adams, and Major General Williams.

[5]Formerly assistant to the President, U. of Rochester. He became President of Purdue U. in January, 1946.

could not be obtained because appointments proposed (mainly contractors' personnel) violated the principle[6] of conflicting interest.

Hovde was therefore named Acting Chief for Section L in place of Dr. Lauritsen, and Hickman continued as Chief of Section H, but of course was ineligible to vote on matters involving the Bell Telephone Laboratories contract, as he remained on its staff, serving the government on a WOC (without compensation) basis.

This organization of Division 3 remained until the end of the war. Watson later withdrew and L. P. Hammett[7] replaced Kistiakowsky. The final list of members was:

C. C. Lauritsen, Director of Research, Contract OEMsr-418, Section L, Division 3
A. Ellett, Chief, Division 4, Ordnance Accessories
R. C. Gibson, Director of Research, Contract OEMsr-273, Section H, Division 3
C. N. Hickman, Chief, Section H, Division 3
L. P. Hammett, member Division 8, Explosives
J. T. Tate, Chief, Division 6, Subsurface Warfare.

The Chief of Division 3 was a member of Divisions 4 (Ordnance Accessories), 5 (New Missiles), 19 (Miscellaneous Weapons), and an NDRC representative on the Joint Chiefs of Staff Committee on New Weapons and Equipment, serving as Chairman of its Rocket Subcommittee; and in this way maintained useful contact with other work of importance to the Rocket Ordnance Division.

The Division 3 staff consisted at this time of three Technical Aides, E. B. Bradford,[8] who served as the Division Executive Officer, and for Section H, J. H. Folse and P. T. Kirwan. Folse transferred to NDRC from the Ordnance Department where he had been engaged in rocket-engineering work. With Section H he specialized in design problems. Kirwan, who had been with Section H since December 1942, engaged first in procurement and expediting activities, next prepared a widely circulated news letter of rocket and related activities based on reports from many sources both in and out-

[6]NDRC appointees were not permitted to vote on matters affecting the contracts of organizations of which they were employees. As all the West Coast program was carried on under the one contract with Cal. Tech., most of the people conveniently located and best qualified to direct the rocket-research program were ineligible to participate in committee recommendations regarding the extent of work to be undertaken on the contract. At a later date, steps were taken to set up a Section L committee of persons not connected with the contractor, but by that time plans were underway looking toward the transfer of the responsibility for rocket research and development to the new Naval Ordnance Test Station (NOTS) at Inyokern, California; so it was believed desirable that any such committee should derive its function from the authority operating that facility.

[7]Professor of Chemistry, Columbia U.

[8]Bradford became Acting Chief when Hovde entered upon his duties as President of Purdue U. in 1946.

side of NDRC, and later devoted part of his time as staff member of the OSRD Liaison Office, concerned particularly with Division 3 contact with the London Mission.

At later dates, additional Technical Aides[9] joined the staff. G. H. Hoppin was added to the Section to handle the administration of[10] its several contracts and assist in their co-ordination.

F. W. Cummings was assigned to Division 3 by the OSRD Engineering and Transition Office, and for a while was Acting Chief Engineer at the Allegany Ballistics Laboratory; B. M. Norton, of Section L, after a short term in the Washington Office, was sent to Pasadena as resident NDRC officer for the Cal. Tech. contract; Grace L. Hart transferred from Division 1 to be in charge of Division Reports; and in 1945 R. S. Warner (formerly a Technical Aide with Division 12) joined Division 3 for special work in connection with the Manhattan Engineer District Project, and was stationed at Los Alamos, New Mexico.[9] Commander D. E. Lane served as Patent Adviser.

The Section H Headquarters were first at the National Academy of Sciences, Washington, D.C. When Tate was Chief, the Division 3 offices were located on the 50th floor of the Empire State Building, New York City, with a suboffice in Washington; then, after Hovde became Chief, in the Carnegie Institution Building at 16th and P Streets, N.W., in Washington, D.C., and in May 1944, were moved to the George Washington University on 21st Street, N.W., where Section H now also had its administrative headquarters.

The Section L office was located in the Visitors' Gallery of the Optical Shop on the Cal. Tech. campus in which the 200-inch mirror for the Mt. Palomar telescope awaited postwar resumption of grinding operations.

The basic organization of OSRD and NDRC, under which Division 3 operated, is not a matter for this story. The principles and methods of operation are described in detail in a companion volume, *Organizing Scientific Research for War.*

[9]G. H. Hoppin, engineer, was stationed in Washington as representative of Shreves, Anderson, and Walker of Detroit. He had previously been resident engineer on the construction of the Dickson Gun Plant. F. W. Cummings, engineer, came to OSRD from the Naval Ordnance Laboratory where he was a production engineer. Before the war he had been a Sales Engineer with a General Motors Delco subsidiary.

B. M. Norton, Professor of Chemistry at Kenyon College, earlier had participated in a Division B Project on HE Research. Miss Hart was for a short period a technical administrative assistant at the Geophysical Laboratory of the Carnegie Institution of Washington and earlier had been a technical research librarian with the Sealtest Corporation. R. S. Warner, industrial chemist and engineer, had served as a technical supervisor with the B. F. Goodrich Co. He was one of the civilian scientists assigned to assemble the atomic bomb in the Pacific and observe the attack on Hiroshima.

[10]The Administration had formerly been in the hands of Dr. Gibson, as Section H Deputy Chief; he had now become Director of Research at the Allegany Ballistics Laboratory on the GWU contract.

SUMMARY OF DIVISION 3 CONTRACT INFORMATION

Amount given includes original figure plus supplements

| *Contractor* | *OSRD Contract No.* | *Amount* | *Subject Work (Short Title)* |
|---|---|---|---|
| Navy—BuORD | | $ 125,000 | |
| GWU | OEMsr-273 | 8,146,300 | Further Rocket Ordnance Development |
| Western Elec.—B.T.L. | 256 | 510,000 | Continued Development of Rocket Accessories |
| Hercules Powder Co. | 337 | 23,000 | Furnishing Powder |
| Hercules Powder Co. | 520 | 2,000 | Furnishing Powder |
| Hercules Powder Co. | 416 | 50,000 | Provision of Experimental Lots of Powders of Special Composition |
| U. of Minn. | 716 | 127,200 | Further Rocket Propellant Research |
| U. of Wisc. | 762 | 30,500 | Further Experimental Investigation of Theories of Rocket Propellant Burning |
| U.S. Army—Ord. Dept. | | 50,000 | |
| Budd Ind. Heating | 671 | 300,000 | Further Production of Special Projectiles and Supplying Related Development Engineering Services |
| Budd Wheel Co. | 968 | 2,310,000 | Further Rocket Engineering |
| Catalyst Res. Corp. | 947 | 7,500 | Development and Production of Special Projectile Accessories |
| Duke U. | 733 | 32,500 | Experimental Studies of Rate and Mechanism of Burning of Rocket Propellant Powder in Closed Bombs |
| Cal. Tech. | 250 | 200,000 | (See 418) |
| Cal. Tech. | 418 | 80,424,000 | 1. Development, Adaptation, and Test Ordnance Devices, Antisub Devices, and Ordnance Materials<br>2. Experimental Production of Such Devices and Materials |
| Cal. Tech.[11] | 702 | 12,000 | Powder Studies |
| | | $92,350,000 | |

[11] OEMsr-702 (L. C. Pauling, official investigator) was not carried on for long under Division 3, but was closed out and the work continued under a new contract with the Explosives Division (8) of NDRC.

Under OSRD, Division 3 was responsible for contracts totaling over $90,000,000, including those made when Section H was a part of Division A. These are tabulated in the facing table.

The amounts shown represent sums appropriated which could exceed in small degree the amounts finally expended. For example on OEMsr-418 there will be a balance when final accounting is complete of the order of $8,000,000. This amount represented only two months' commitments at the rate work was proceeding just before V–E Day.

The funds allocated to these contracts came from OSRD appropriations, and by transfers to OSRD from the Services to support some of the large-scale programs. In the case of interim pilot production between adoption of a round or device and direct Service procurement from prime contractors, the costs were covered by Service-transferred funds.[12]

It was a requirement of the Director of the Budget that funds appropriated by Congress for research and development should not be used to supplement funds authorized for Service procurement. It was therefore particularly essential that the Services support OSRD pilot-production activities by transfer of funds in advance when need existed for getting new devices into the war theaters for final testing under field conditions.

It could be stated that OSRD's development job was not done until adequate operational tests had been made — and in the case of ammunition large quantities of rounds would be required. Certainly the handling of preliminary procurement through the organizations that had developed the devices or weapons was a real economy of time in the over-all war effort. In accepting Service funds for support of its contracts, OSRD made it clear that it was not serving as a procurement agency providing items to meet Service specifications. Production was under the direction and control of the research staffs and was carried out to complete the development of weapons — first Service use could be an important part of such a program.

The Navy transferred to OSRD for support of the work under Section L a total of $57,000,000. This was not limited to procurement but was for support of the rocket program in general. The cost of ammunition and equipment delivered was approximately $47,500,000, exclusive of $8,000,000 in cost of plant facilities and materials on hand at the end of the war, most of which were transferred to the Navy for its peacetime program.

The following list is representative of the projects assigned to Division 3. A total of 65 was undertaken by its contractors, although this does represent some overlapping, because one Service might establish a project merely for the purpose of indicating its interest and having formal liaison with projects originating with the other.

[12]In the case of the larger contracts, advance payments were made to the contractor as recommended by the Supervising Division.

1. Rocket propellants and ballistics
2. Jet acceleration for AP bombs
3. Rocket targets
4. Beach barrage rocket weapons
5. Hydrodynamic characteristics of projectile forms (with Div. 6)
6. Underwater trajectories of depth charges (with Div. 6)
7. Torpedoes for high-speed aircraft (with Div. 6)
8. Forward-firing 5-inch aircraft rockets
9. Spin-stabilized rockets
10. "Window" rockets ("Window" is an anti-radar screening technique)
11. Development of 2.36 high-velocity rocket (super-bazooka)
12. Pressurizing flame throwers
13. Minefield-clearing devices
14. Design, construction, and operation of extrusion presses (China Lake Pilot Plant, NOTS, Inyokern)
15. Assistance to the Naval Ordnance Test Station, Inyokern
16. Development and fabrication of launching rockets for guided missiles

Some of the projects had specific extensions to cover crash production.

As an example of a crash-production program let us consider what happened in the case of 5-inch rockets, in 1944.

Early in September, the Bureau of Ordnance requested Section L to continue motor production for 5-inch high-velocity aircraft rockets, that in June–July[13] of that year had proved to be so effective as an antitank weapon, to a total of 65,000, at a rate of 500 per day.

The California Institute had already carried through successfully a number of crash procurements, but, of course, its function was not to be a prime producer of ammunition. In the subsequent discussions it was made clear that a program of this kind was an essential initial procurement, and not a continuing production of standardized rockets to supplement that of Navy contractors. If the pilot production of new rounds could be under the direction of the research and development people, it would be easier to iron out manufacturing difficulties and to make adjustments at a period when time was vital. The California Institute of Technology would furnish materials, obtained under Navy priorities, to subcontractors for fabrication of rocket components, and then perform expediting services and inspection and assembly operations.

No sooner had this expanded aircraft-rocket crash-procurement program begun than an urgent request came from the Bureau of Ordnance to deliver 74,000 spin-stabilized rockets, designs for which were hardly out of the experimental stages. But this new type would increase significantly the usefulness of rockets fired from light craft in landing operations.

It took nearly a month more to work out the production kinks. Increased

[13]See Chapter XXI.

space had to be acquired close by. This last was accomplished by the Office of Scientific Research and Development through condemnation proceedings carried out by the Los Angeles Office of the Attorney General of the United States, which acquired the adjacent plant from which the installed equipment was moved away to a new location within three days, sooner than Judge Leon R. Yankwich had allowed the owners to make the place available for the OSRD contract work. This quick move was possible because of the excellent co-operation between the officials of the owning company and the Developmental Engineering Section of the OSRD–C.I.T. contract. [Note that the abbreviations "Cal. Tech." and "C.I.T." are used interchangeably throughout the OSRD history series.]

The schedule, based on commitments made to the Chief of Naval Operations by Bureau of Ordnance, called for shipping 45,000 SSR's from C.I.T. by December 15. In this rush program the launcher production for the SSR rockets was completed[14] by direct Navy contract with the Consolidated Steel Company with the California Institute of Technology engineers A. S. Gould, C. V. Heath, and R. E. Sears assisting. The latter, a member of the launcher group of the Design Section of the contract organization, followed the landing craft, on which the launchers were installed, to Pearl Harbor, where final adjustments (needed because of not unusual difficulties experienced in first large-scale production) were made.

In the rocket motor program the first week's output was rejected due to faulty contact rings; and the middle of November found production about a week behind schedule. But procurement of the rounds was up to over 1000 per day by December and on December 15 the score of deliveries was 45,000 rocket motors and 46,000 heads, a splendid job accomplished by the 1000 persons of the Developmental Engineering Section, headed by Trevor Gardner.

These rockets, which extended the beach-landing barrage rocket range from 1000 yards off shore to 5000 yards, provided cover[15] at Iwo Jima in February 1945, and later on the Okinawa landings.

Successful and rapid execution of NDRC rocket research and development was greatly facilitated by knowledge of what the British had done. Rocket liaison for Division 3 gained by the fact that Hovde, later Chief of the Division, was first resident secretary of the OSRD London Mission. Representatives of this mission worked closely with American Military and Naval attachés and other agencies, and obtained information on the development of new and improved rocket and jet-propulsion devices.

NDRC divisions were often represented by resident members of the

[14] First experimental launchers were built under Cal. Tech. subcontract with the Nigg Company.

[15] An account of this will be found in Chapter XXIII.

mission and were also represented on the staff of the Washington Liaison office which provided for the exchange of information between workers in this country and in Britain. Division 3, however, was not represented in London by a regular staff member of the mission until the arrival in August 1944 of Dr. Daniel B. Clapp,[16] who represented also Divisions 8 and 9.

From March 1941 until May 1942, Hovde kept NDRC generally informed concerning British rocket work. The May–July visit of C. C. Lauritsen in that year as already described led to great expansion in the NDRC rocket-research program. Other OSRD scientists who reported from London, concerning British rocket research in the period May 1942–February 1944, were Drs. T. R. Hogness, J. R. Johnson, and J. H. Hildebrand, representing the chemical divisions, and R. F. Simonds of Division 6. Dr. L. G. Straub, Technical Aide with Division 6 (and formerly also with Division 3), although not on the London Mission staff, was resident in London from February through July 1944; he reported to Division 3 on British rocket developments and also on use of rockets by British and American forces.

John Beek of Section H and W. P. Huntley of Section L, Contract OEMsr-418, were abroad in 1945 on special missions under assignment to the London office. Beek was concerned with propellants and Huntley with underwater characteristics of projectiles.

A number of British representatives kept in close touch with the American program. The interchange of visits afforded firsthand information in keeping Division 3 and its contractors abreast of British developments.

Under arrangements made with the Office of Field Service of OSRD, Division 3 workers at the request of the Army or Navy made trips as Technical Observers and Field Service or Scientific Consultants to the theaters of operation for the purpose of assisting in the use and evaluation of the weapons in the development of which they had participated, and for special missions for which their training and experience fitted them. Some of these expeditions[17] are described in the text in connection with the weapon stories.

[16]Assistant Professor of Chemistry at Williams College.

[17]A summary follows; in some cases, as indicated, the arrangements for the visits were made directly by the Army or Navy.

| Theater | Year | Name | Division 3 Position |
|---|---|---|---|
| Pacific | 1945 | R. V. Adams | Supervisor, Aircraft Ballistics Group, Inyokern Range Operations Section, Section L, OEMsr-418 |
| European (Army) | 1944 | C. D. Anderson | Supervisor, Aircraft and Ballistics Section, Section L, OEMsr-418 |
| So. Pacific | 1944 | W. A. Fowler | Asst. Director of Research, Section L, OEMsr-418 |

We may now follow the Eastern and Western Divisions, one at a time to the end of their trail. The reader must realize that this is done for clarity and his convenience, and does not impute greater importance or earlier accomplishment to the one of two parallel developments which is described first.

| | | | |
|---|---|---|---|
| So. Pacific (Navy) | 1945 | J. L. Fuller | Research Staff — Fire Control Group, Aircraft and Ballistics Section, Section L, OEMsr-418 |
| European | 1944 | C. N. Hickman | Chief Section H. Division 3, NDRC |
| European (Army) | 1944 | C. C. Lauritsen | Director of Research, Section L, OEMsr-418 |
| S.W. Pacific | 1945 | P. E. Lloyd | Research Staff—Land/Amphibious Launcher Group, Design and Development Section, Section L, OEMsr-418 |
| Pacific | 1945 | L. H. Mahony | Research Assistant, Projectile Group, Design and Development Section, Section L, OEMsr-418 |
| Pacific | 1945 | F. T. McClure | Supervisor Theoretical Ballistics, Section H, OEMsr-273 |
| European (Army) | 1945 | A. L. Melzian | Asst. Supervisor Aircraft and Ballistics Section, Section L, OEMsr-418 |
| S.W. Pacific | 1944 | L. A. Richards | Supervisor Land/Amphibious Launcher Group, Design and Development Section, Section L, OEMsr-418 |
| Hawaii Pacific (Navy) | 1945 (2) | R. E. Sears | Research Staff Land/Amphibious Launcher Group, Design and Development Section, Section L, OEMsr-418 |
| Pacific (Army) | 1945 | R. S. Warner | Technical Aide, Division 3, NDRC |
| European | 1945 | J. H. Wayland | Research Staff, Torpedo Launching Section, Section L, OEMsr-418. |

# Further History of the Eastern Group

## CHAPTER VIII

## *SECTION H AFTER THE REORGANIZATION*

FOR SOME TIME after the reorganization Section H continued to carry on its activities at Indian Head. But by the summer of 1943 the facilities and conditions there were no longer adequate, while the Navy felt its own needs prevented any further expansion of the facility. Dr. Van Evera, Official Investigator of Contract OEMsr-273, therefore set out to find a new location.

The first of the possibilities was opposed by local fishermen, who had enough senatorial influence to make a move there inadvisable. A second was selected and leased across the Potomac from Indian Head, but before any move was made another and more suitable location was found.

On the north bank of the Potomac River, ten miles west of Cumberland, Maryland, is a 438-acre tract bounded by the river on the north and east and by 700-foot hills on the south and west. There the Ordnance Department of the Army had established a facility, then known as the Allegany Ordnance Plant, Pinto Branch. It had been used for the loading and assembly of .50-caliber ammunition. On December 28, 1943, the Army issued a permit to OSRD to use this plant as a rocket-development laboratory. Many of the former Ordnance plant buildings were well adapted to the needs of the new establishment, and in all about fifty of these were used. The Laboratory was named the Allegany Ballistics Laboratory (ABL) and was operated for Section H by George Washington University.

Although there were no facilities at ABL for long-range firing, this was not a serious drawback, for by this time the Section H workers had learned that practically all the factors affecting the accuracy of finned rockets were operative only during the burning period, a relatively small part of the whole flight. It was found that firings into the hillside from a distance of 1000 or 2000 yards provided adequate data. The laboratory also operated a long-firing range located on Allegany Mountain about forty miles south of the main site.

Much of the experimental work in the Section H rocket developments described in subsequent chapters was carried out at ABL and by ABL people at Army establishments. Most of the theoretical and administrative work of the contract remained in Washington in university buildings. Members of the research staff were drawn from universities and industrial research organizations located in all parts of the country. Most of the plant operational staff came from local communities. The total number of employees reached a maximum of 700 in August 1945, with the research staff made up of about 125 chemists, physicists, mathematicians and engineers.

Chart X (pages 46–47) shows the organization for Contract OEMsr-273 with George Washington University as of August 1945 and its integration through Division 3 with OSRD. It will be noticed that a number of the staff were among those originally holding NDRC appointment as consultants. Dr. Gibson, the Director of Research, had, in addition, been Deputy Chief of Section H, and Dr. Kossiakoff, Deputy Director, had been a Technical Aide. Alfred Africano, Assistant to the Director, was the first engineer employed. He was president of the American Rocket Society, 1937–40. A more extensive personnel list is provided in Appendix 1a (pp. 217–218).[1]

On the basis of proposals by divisions, recommendations by NDRC, and approval by the Director of OSRD, the Contracting Officer of OSRD made war-research contracts with educational and industrial organizations. Certain persons (Division Chiefs, Section Chiefs, and Technical Aides) were named in the contracts as the authorized representatives of the Contracting Officer for the purpose of providing official instructions to contractors regarding the subject work.

In the spring of 1944, this was changed to name in contracts a Scientific Officer, normally the Division Chief, and perhaps the Section Chief and one or more Technical Aides as assistants, who would be responsible for supervision of the scientific work of the NDRC–OSRD contracts. The Scientific Officer and Assistants were the official representatives of the Government in technical matters.

The following list is illustrative (although not necessarily typical) of some of the functions the staff of a division performs in carrying out its responsibilities in providing technical supervision of contracts: (1) Preparation of contract proposals; (2) keeping budget records; (3) making investigations and recommendations in connection with acceptance, assignment, initiation, and extension of projects; (4) acting as NDRC project officers; (5) approving subject matter of contractor's reports, accepting for NDRC, and supervising distribution; (6) issuing or relaying instructions to contractor on subject work; (7) checking and supervising contractor's

[1]Appendix 1b (pp. 219–220) lists the principal personnel of Section H contractors' organizations other than that of GWU.

fulfillment of obligations with respect to reports, conduct of work, deliveries to Services, invention reports; (8) making recommendations to Chief of Division on approval of research subcontracts; (9) approving and certifying as necessary for prosecution of the program and forwarding to Contracting Officer appropriate contractor's requests to purchase real estate, construct and alter buildings, purchase motor vehicles, etc.; and (10) reviewing reimbursement and advance-payment vouchers, and providing, if proper, *technical* approval of such vouchers in accordance with OSRD regulations.

NDRC generally gave instructions with respect to conducting the subject work to its contractors in broad terms as it was attempting through its contracts to secure the best available talent for directing and carrying out the research and development on assigned projects.

Close liaison was maintained between the NDRC activities at the laboratory and the Services. The Office of the Chief of Ordnance operated a suboffice at ABL under Major J. F. Miller, Jr., and later Captain H. H. Abrams. These officers had direct participation in research and development activities. The Navy Liaison Officer, Lieutenant J. J. Donovan, was also in charge of the laboratory research in thermochemistry. The Chemical Warfare Service maintained the Pinto-CWS Group, under Major A. R. T. Denues. In turn ABL maintained a liaison officer, T. J. McCormick, at the 4146th AAF Base Unit (ATSC) at Dover, Delaware.

Although George Washington University was the largest of the Section H contractors[2] and conducted the work of greatest importance at ABL, there were five other contracts for all or most of the war period. These were with the Budd Wheel Company, the largest contract, with the Western Electric Company, with Duke University, and the Universities of Minnesota and Wisconsin.

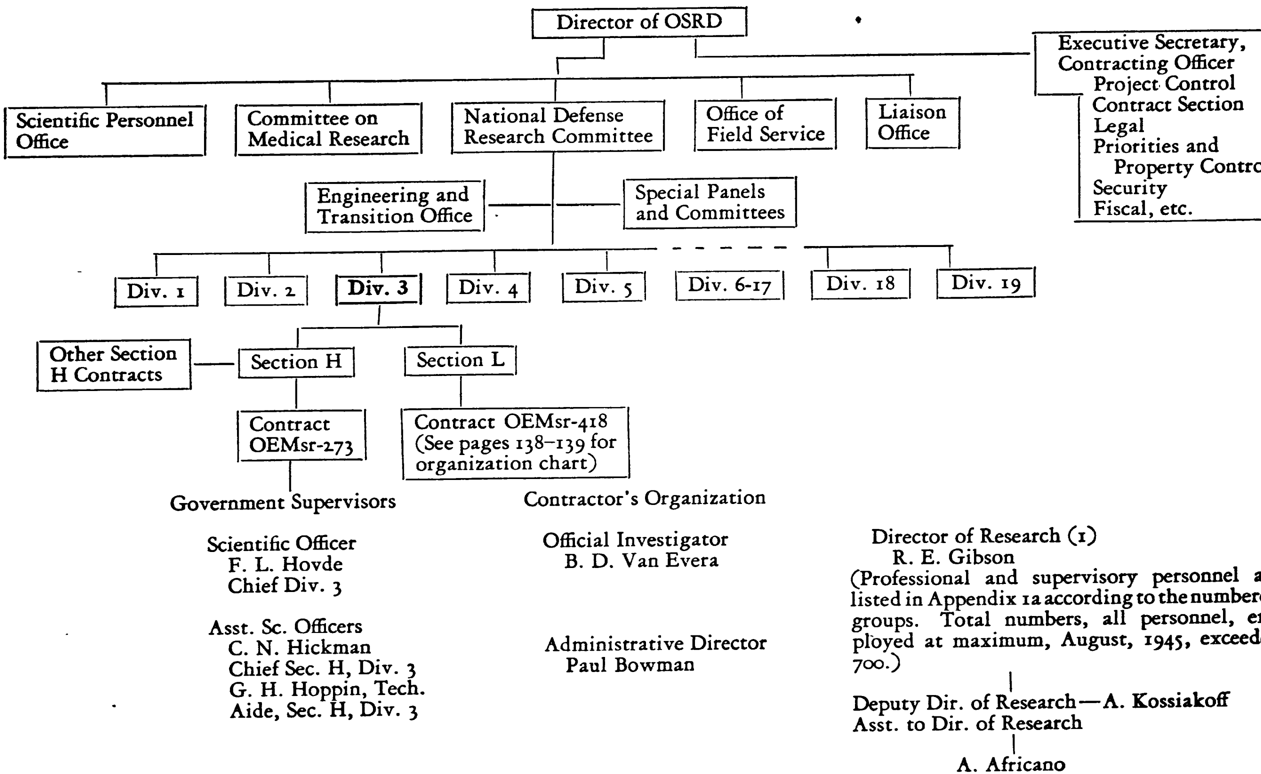
Budd, under the direction of William Farr and W. B. Pope, operated divisions of heat treating, strength testing, design and engineering (including, by subcontract, assistance from Special Engineering Service, Inc., of Detroit), and a small production-line department which furnished first models for field test. Later, as medium-quantity production of parts and whole units for proof tests were supplied by subcontractors, Budd supervised them through its design and engineering group.[3]

Western Electric provided the services of the Bell Telephone Laboratories on a variety of problems for which the great research organization was well suited. Starting with instrumentation problems, mainly, this contribution developed in several fields. The Bell Labs did much work on igniters for the 4.5-inch rockets and produced a bayonet-shaped one, of

[2] Appendix 2 (p. 226) lists GWU subcontracts involving research and development.
[3] In addition, the Budd Rocket Division included production under direct Army Ordnance contracts of rocket components developed by Section H.

# Chart X.

## ORGANIZATION FOR CONTRACT OEMsr-273 WITH GEORGE WASHINGTON UNIVERSITY



- **Powder Operations (2)** — T. S. McCormick, Chief
  - Powder Service
  - Loading Operations
  - Powder Machining and Igniter Service
- **Rocket Weapons Div. (3)** — A. Kossiakoff, Chief
  - Ballistic Performance Sec. I
  - Special Proj. Group
  - Flight Operations
  - Analysis
  - Ordnance
  - Ballistic Design, Sec. II
  - External Ballistics Sec. III
  - Propellant Applications, Sec. IV
- **Engineering (4)** — A. S. Glenn, Chief
  - Planning Dept.
  - Proj. Engineers
  - Design Eng., Dept. I
    - Design Eng. Sec.
    - Drafting Sec.
  - Prod. Eng. Dept. II
    - Procurement Sec.
    - Mfg. Sec.
    - Material Control Sec.
  - Production Control Dept.
- **Instrument Division (5)** — N. E. Alexander, Chief
  - Static Range Sec.
  - Flight Range Sec.
  - Device and Construction Sec.
  - Gauge Calibration Sec.
- **Propellant Division (6)** — W. H. Avery, Chief
  - *A* Range
  - *B* Range
  - *C* Range
  - *D* Range
  - Thermochemical Sec.
  - Extrusion and Physical Sec.
  - Propellant Production Problems
  - Igniter Research
- **Recoilless Weapons Division (7)** — C. N. Hickman, Chief
  - Section I
  - Section II
  - Section III
  - Bear Rocks Range
- **Project Co-ordinator (8)** — J. Burns, Information Sec.
  - Stenographic Pool
  - Library
  - Photographic
  - Photocopy

which several million were in Ordnance Department production by the end of the war; under R. F. Mallina they did a great deal of work on fuzes and on lightweight single-tube, multiple-tube, airplane, and special lightweight launchers.[4] A number of accessories were designed there, including ripple-firing controls, special timing switches, pressure-relief valves, holding devices, igniters, nozzle closures; and the laboratory also contributed several pieces of specialized equipment including a high-speed ribbon-frame camera,[5] recording mechanisms for static testing, copper-ball pressure-gauge elements, amplifiers, and an amplifier calibrator for use in the electronic recording of thrust and pressure.

The three university contractors, other than GWU, were closely allied and directed towards studies in propellants. Their combined results were correlated, applied, and extended through their association with the Allegany Ballistics Laboratory. Thus Professor Farrington Daniels at Wisconsin developed a comprehensive explanation of powder burning; Dr. B. L. Crawford at Minnesota worked in a region intermediate between the basic work of Dr. Daniels and the applications at ABL. In addition to work on stabilization of burning of high-power propellants, he perfected an apparatus for the rapid visual observation of burning rates and correlated the results with those obtained in actual rockets. Dr. L. G. Bonner directed the work at Duke which made calorimetric determinations on heats of explosion. On the termination of this contract Dr. Bonner and several of his assistants transferred to ABL.[6]

A further chronological separation of work at Indian Head from that into which it developed at ABL will serve the purposes neither of understanding nor of continuity. While still at Indian Head, members of the Section had a number of different and quite diverse problems. They continued work on the armor-piercing bomb and succeeded in developing a jet-accelerated AP bomb not only for level flight but also for dive bombing. As was to happen on other occasions during the rocket program this rocket-propelled-bomb project turned out to be a source of experience but not a military sensation. A tactical opportunity to make use of the weapon did not develop. Work on the 4.2-inch chemical mortar was successful and had a still more important end product, so the story of this work can best be combined with that story in Chapter XI.

Tests on target rockets fitted with plywood fins were made in September 1942. These rockets could be provided with large fins and the rockets fired to simulate low-flying attack planes. The Ordnance Department took over

[4]Under direct contracts with Army OD and CWS, the Western Electric Company engineered and manufactured firing systems for several types of multiple rocket launchers.

[5]This was similar in principle to the original Cal. Tech. model which Hickman had seen at Pasadena; but was lighter, more portable, and less expensive.

[6]Dr. Bonner, when operation of ABL was transferred to an industrial contractor at the end of the war, remained as Technical Director.

the development in that month and before the end of the war the Army used many thousands in its air-training program.

Finally in the anxious moments when we began to know of the probable existence of V-2 but not enough about it, the Section H experts were called in, with others, for advice. Their contribution to the effort to emasculate the V-2 is not a part of this story, but it is enough to know that because of their experience in rocketry they were able to make very close predictions as to what a V-2 would be like. There is no doubt that this country could have developed a V-2, had it had a military purpose for us, although the many years of fundamental rocket work in Germany surely would have made the Nazi progress easier than ours. For this work Messrs. Gibson, Rosser, Hickman, and McClure were commended by Major General Stephen G. Henry in a letter of March 14, 1944, to Dr. Bush.

It may be worth reflecting that we had considered this German secret weapon long before they ever used it in combat, and could have built it eventually if we had felt the development warranted. Admittedly we were lagging behind them in the knowledge and research[7] necessary for the development, but a year or two of major effort probably could have produced the same weapon for the Allies if it had been considered necessary. Imaginative developments are the stock in trade of normal scientific progress and no nation has a monopoly on imagination. The history of science includes many imaginative developments which have occurred separately and simultaneously in widely scattered places on the globe.

But the achievements of the eastern section of Division 3 can best be understood by consideration of the individual weapons with which they were concerned. These were, in the order in which they will be discussed, the bazooka, the 4.5-inch aircraft rocket, the recoilless gun, improvements in the flame thrower, pusher rockets, and jet-assisted take-off for airplanes. To them we may now turn.

[7] It is of interest to note that three of the features developed by Goddard earlier appear in the V-2. (1) Pumps were used in injecting the fuel into the combustion chamber. (2) Gyroscopically controlled vanes were used within the nozzle for stabilizing the rocket up to the point where the air resistance could be made use of by external vanes. (3) An excess of alcohol was fed in so that a blanket of gas was placed between the combustion point and the walls of the motor, thereby preventing it from becoming excessively hot.

# CHAPTER IX

## *BAZOOKA*

WE NOW return to the story of the work at Indian Head, beginning in the summer of 1941. This may be divided into two periods: the first at Indian Head, Maryland, and the later one at Allegany Ballistics Laboratory (ABL) in West Virginia. The first covers the period 1941–43. Section H moved to ABL at the beginning of 1944, and work still continued there at high intensity at the end of the war.

In August 1941, the Navy stated that NDRC's part of the development of the jet-accelerated AP bomb was complete, and it was up to the Bureau of Ordnance to carry on. Actually, however, Section H continued work on this for some time.

In November, under the direction of Dr. Jesse DuMond,[1] a press for dry extrusion was set up which produced grains for experimental purposes. At first the powder grains contained cracks. In December, tests showed that solventless-extruded grains had less mechanical strength than solvent-extruded, resulting, in the Indian Head experiments, in breakup of the grains during burning.

In 1941 Section H and the Army Ordnance Department began their cooperation on the project which was later to produce the bazooka. The Ordnance Department had a three-pound shaped-charge[2] projectile head capable of perforating the armor of most tanks then in use. This was accomplished without the need for high impact velocity. The problem was to provide the infantryman with means for projecting this head, nose first, against a moving tank from a reasonable distance and with suitable accuracy.

The requirement for nose-first impact eliminated the possibility of hand throwing. Projection as a rifle grenade, from shoulder rifles and even from .50-caliber machine guns, was found to be impracticable because of inade-

[1]Dr. DuMond, associate professor of physics, Cal. Tech., was engaged in investigations for Section E of Division A. At the end of 1941 he returned to Pasadena to continue work with the Cal. Tech. group and Dr. Gibson undertook closer supervision of the dry extrusion at Indian Head.

[2]In 1888, the American explosives chemist, Charles Edward Munroe, discovered that if the surface of a block of high explosive is indented in spots, and then exploded with this indented surface against a steel plate, the explosive effect on the steel plate is greatest opposite the points of indentation.

The shaped-charge head, designed by a Swiss engineer named Mohaupt, is an application of this principle. A cone-shaped cup hollowed in the front face of the explosive filling focuses the blast energy into a narrow beam of great penetrating power.

quate range and accuracy and excessive recoil. The lack of recoil which characterizes rocket propulsion seemed to promise that the shaped-charge head might be projected from a light weapon.

In an attempt to avoid burdening the infantry with a special-purpose weapon the Ordnance Department specified that the rocket, with the shaped-charge head, should be launched from special fittings on the Service rifle. Soon the short range, inaccuracy, and blast difficulties of this method were demonstrated. Trials were then made (on the workers' own initiative) of launching through an open-ended tube over the operator's shoulder as originally recommended by Hickman.

The early rocket grenade fired from the tube launcher was not satisfactory because a wrong propellant granulation was used. The workers were still using a single 7/8-inch stick of powder with a web of some 6/15 of an inch; that is, this thickness of powder had to be burned through. The burning took up too long a time and continued over several feet of initial rocket flight — well beyond the practical length of any tube launcher. Thus when the rocket left the end of the launcher, blast was still coming out of the back, and such hot gases would certainly scorch the face of the gunner. Moreover, this long burning distance caused inaccuracy. A rocket is more accurate when it is guided in a launcher-tube during the entire time of burning. It is then good design for accuracy to have burning distance no longer than the length of the tube. It was clearly desirable to reduce the burning distance of the bazooka projectile. This was accomplished by decreasing the web and increasing the burning rate. The thinner web was achieved by changing from 7/8-inch tubular powder grains to grains of 3/8-inch-outside and 1/16-inch-inside diameter. Five such grains fitted nicely in the rocket motor. The increase in burning rate was achieved by operating a higher pressure, the nozzle size being changed to do this.

These factors had to be adjusted so that the chamber pressure would not be too high — some 500 atmospheres (7500 pounds per square inch) was about right — and so that the total amount of the propellant would give the rocket grenade the desired range of five or six hundred yards and the desired muzzle velocity of about 300 feet per second. The burning time had to be kept down to some thirty thousandths of a second. All this required a considerable amount of careful balancing of one factor against the next, and a large number of trial-and-error experiments, each one indicating further refinements which would lead to the desired end.[3]

At the same time, since the idea of launching from a rifle had been abandoned, it was necessary to design a launcher. Basically it was to be a simple tube, just large enough to allow the rocket grenade to slip inside it and pass through freely. It had to be designed with grips so that the

[3]Sidney Golden, a George Washington University contract employee, supervised the ballistics test that eventually standardized the charge.

soldier could hold it on his shoulder. More important than this, some firing mechanism had to be designed which would be reliable and rugged. Both percussion and electrical firing systems were tried; the electrical was finally decided upon. The launcher was equipped with a suitable ring which made contact with a mating part when the projectile was slipped into the rear end of the launcher. When the infantryman squeezed the trigger, the circuit was completed, an electrically operated igniter in the projectile was fired, and the rocket grenade was on its way to destroy an enemy tank.

After many successful firings at Indian Head, including the first shoulder firing, Lieutenant (later Major) E. G. Uhl went to Aberdeen Proving Ground to demonstrate the weapon, expecting to fire a few rounds from his experimental tube launcher held in a clamp. But a tank was available for the test at Aberdeen, and it was suggested that Major Uhl fire the weapon from his shoulder at the moving target. He had no range data, the experimental launcher had no sight, and no shoulder shots at a moving target had ever been fired. With only a rough idea of the range from experimental tests, he made a few hasty calculations of gravity drop and improvised a sight from a piece of wire and an old nail found on the ground. In the first test with this crude sight he aimed at a moving tank, fired, and successfully restrained himself from showing his relief when he made a bull's-eye.

The urgent need for the weapon finally resulted in a waiver of the requirement that the grenade be fired from a rifle, and the tube method of launching was adopted. Work along these lines eventually led to the present type of tube launcher known as the bazooka. All of the difficulties, however, were not settled by the tube launcher. Neither the accuracy problem nor that of the blast was solved so easily.

For one thing, it was desirable to have the launching tube as short as possible so that it would be easy to carry. To select the proper length, a 6½-foot launcher was built—everyone was sure this would be longer than the burning distance—and a number of holes were drilled through the side toward the front end. Ordinary Scotch tape was pasted over each of these holes, and a rocket grenade was fired. Some of the tape-covered holes toward the front were not blackened since the burning was all over by the time the projectile reached these holes. The first unblackened piece of tape was over a hole about 4½ feet from the rear end of the launcher, and this distance was taken as the final launcher length.

The complete rocket grenade was demonstrated at Camp Sims to the Army boards. It had muzzle velocity of 300 feet per second, and an extreme range of some 600 yards. The accuracy, while not phenomenal, was more than sufficient for hitting a tank at 100 or 200 yards. Although a special launcher was required, this piece weighed less than fifteen pounds and was easily carried.

The rest of the story is known to the public. The Indian Head Laboratory turned over its drawings of an acceptable bazooka model and launcher to the Ordnance Department. The Army and industry co-operated in producing the ammunition and launchers in quantity in time for the invasion of North Africa. There the German Panzers met defeat. The first American rocket weapon had played a decisive part in winning a critical battle of this war.

The bazooka was a success; but it was not a perfect weapon. For quantity production of the motor chamber, the Ordnance Department specified a somewhat weaker steel, more readily available and easier to fabricate than the very tough steel of the experimental model tube. The new steel chambers were perfectly good at ordinary temperatures, but they did give the temperature problem a chance to cause trouble. When the new bazooka motors were fired on very hot days, the pressure was so great that they blew up. In order to prevent this, the Ordnance Department reduced the weight of the powder used; this successfully reduced the pressure in the chamber and prevented the blowups. But now the other end of the temperature problem caused trouble; at low temperatures — even on a slightly chilly day — the reduced amount of powder surface gave a much lower pressure and consequently a longer burning time. The burning time was indeed so long that the rocket continued burning after it left the launcher tube, and its blast came back in the soldier's face.

Clearly, what was needed was a strong chamber or a better propellant, and both problems were attacked. The Ordnance Department requested the NDRC to develop a better bazooka powder. Division 8 of the NDRC had done a great amount of research and development of propellants, quite different from conventional double-base smokeless powders, and having burning rates less sensitive to temperature and pressure. Experience gained in this work was applied to the bazooka needs, and resulted in a new "blastless bazooka propellant," familiarly known as BBP. This differs from conventional powders in containing 9 per cent of a perchlorate-carbon mixture in a solvent-extruded double-base grain. BBP, used in the stronger motor, eliminates both the high-temperature blowups, and the low-temperature long-burning blasts.

Thus the weapon which helped to scatter Rommel's tanks, broken, over the sands of the Sahara, was brought to a still greater degree of perfection. The NDRC–Army Ordnance team continued to improve the weapons and increase the advantage of the American soldier in the field, by making the bazooka safe and reliable. This was not, however gratifying, a signal to stop.

By the time of the Japanese surrender, a super-bazooka projectile was being made ready for Service use. Fired from the standard shoulder launcher, this rocket, as compared with the earlier ammunition, had

almost twice the velocity, much improved accuracy, an effective range increased from 300 to 700 yards, and the ability to penetrate much thicker armor. The temperature limits for safe operation, both as to high-temperature motor bursts and low-temperature blast outside the launcher, were substantially wider.

The improved head had been developed under Division 8, NDRC. The super-bazooka was developed in Section H. Its improved performance was achieved not merely by increasing the size but mainly by a radical change in the design of the propelling charge. In the original bazooka the charge consisted of several perforated cylindrical grains, of thin web. The super-bazooka charge was made up of many disks of sheet powder, stacked in a tapered column to allow free flow of the gases to the nozzle. This type of charge was developed by Sidney Golden of the George Washington University group; it was suggested by the somewhat similar method of mounting propellant on the shells for the 4.2-inch chemical mortar.

Although the super-bazooka did not see combat action, it constitutes a valuable addition to rocket development work for rockets of the future.

CHAPTER X

# *THE 4.5-INCH AIRCRAFT ARMAMENT ROCKET*

A FIGHTER plane is essentially a gun platform. Whether it is shooting at other planes, or at ground targets, a measure of its military effectiveness is fire power. A plane with .50-caliber machine guns is better than one with .30-caliber guns. But larger-caliber guns bring with them the problem of greater recoil. Probably the limit to mounting standard guns on planes of the day was reached when, with the aid of a very ingenious recoil-absorbing mechanism, a 75-mm. gun was mounted in the nose of a B-25 bomber — and even with this elaborate mounting, pilots reported that the recoil from the gun seemed almost to stop the plane in mid-air. An obvious answer to the recoil problem in aircraft armament lies in the rocket.

So it was not surprising that the interest in rockets for use on planes which had lain dormant for twenty-three years should reawaken. The Army wanted a somewhat more powerful rocket than the 4-inch projectiles developed in 1918 — ones which would carry a larger and more effective explosive charge. And so the Section H scientists, in co-operation with the Ordnance Department, set out to develop the 4.5-inch rocket primarily for use as aircraft armament.

The new rocket as originally designed consisted of a motor tube 4.5 inches in diameter and approximately 2 feet long, one end of which was formed into a nozzle, with a head of the same diameter threaded to fit the motor. Attached to the head was a burster tube 1¾ inches in diameter and 15 inches long, which extended down the center of the rocket motor. On detonation, the high explosive in the head fragmented not only the shell but also the motor, thereby utilizing the entire weight of metal in the rocket and materially increasing its effectiveness against enemy aircraft and personnel.

At this time the Air Forces, fearing damage to plane structures from the rocket's blast, specified that aircraft rockets must be launched from tubes mounted under the wings. The tube launcher would serve to protect the wings of the airplane by enclosing the blast of the rocket. It was, therefore, necessary to limit the maximum diameter of the entire rocket including fins to 4.5 inches. The design of a suitable fin to stabilize the 4.5-inch rocket in flight presented a difficult problem, since, in order to achieve sta-

bility, the fins must project into the air stream. The solution to this problem was found in the development of folding fins, which nested in the necked-in portion of the nozzle and pivoted backward to an extended position as soon as the rocket emerged from the launching tube.

The propellant charge of the rocket consisted of ten columns of tubular powder grains of 7/8-inch diameter, hung from the head end of the rocket by wires. The powder was made by the "new-technique" solvent process.[1]

The evolution of the 4.5-inch medium rocket can be divided into four phases, leading to models which we may distinguish by their Army designation: the original M8, the improved M8 (M8A3), the T22, and the T22 with T23 kit. The history of these developments and improvements illustrates the need for continual effort on the part of Army and civilian scientists to improve ordnance devices in order to achieve their maximum effectiveness against the enemy.

Work on the 4.5-inch rocket began at Indian Head when rocket development facilities were badly lacking. In order to obtain a working model quickly, first test units were made from fire extinguishers. The bottom of the fire extinguisher became the head of the rocket, and its tapered top gave a good start for the nozzle. The necessary modifications — welding in a plate to separate the head from the motor chamber, adding fins, etc. — were made at the Navy Yard. The first range test of these crude but valuable early models was made on May 29, 1941.[2]

A successful firing test of the 4.5-inch rocket was held at Aberdeen Proving Ground for Army officials. The reaction to this rocket was so favorable that the Ordnance Department immediately began to complete standardization of the rocket and made plans to put it into quantity production. Lieutenant Colonel Harry Donicht of Wright Field was assigned soon after as Ordnance Department, NDRC, Army Air Force Liaison Officer. Since he was concerned about blast effects on an airplane wing, one was sent to Indian Head for tests. The 4½-inch rocket was first fired from a plane in flight,[3] a P-40, on July 6, 1942.

At this critical time, when aircraft rockets were badly needed for use against enemy aircraft, a serious difficulty was encountered with the propellant. In order to permit visual inspection of the powder grains to reject

[1]Early in 1942, the propellant problem continued to be serious. Solvent-extruded grains being delivered were not uniform. Dr. Gibson and John Beek, a chemist from the Bureau of Standards who joined Section H as consultant, consulted with Hercules in working out extrusion techniques. Working in cooperation with the Hercules technical staff, improvements were made, and the powder was designated NT (New Technique).

[2]The first production-type models of the M8 were made by the Dresser Manufacturing Company and later by the Revere Copper and Brass Company.

[3]"Army Ordnance Department Contribution to Rocket Story for Rocket Panel of the Joint Board on Scientific Information Policy" — September 1, 1945. Chapter VII, "Rocket Armament for Aircraft."

grains containing flaws, a stabilizing[4] constituent, diphenylamine, which had caused the powder to darken upon storage, was replaced by a substitute, ethyl centralite (diethyl diphenylurea) which left the powder translucent. This modified powder caused motor blowups when fired under apparently normal conditions. Partly burned fragments of powder recovered after these blowups showed the powder to be full of holes, having an appearance as though it had been attacked by termites. An emergency experimental analysis indicated that the phenomenon was associated with the darkness of the powder — light samples of powder giving a greater number of blowups than dark samples of powder of the same composition. Previously, the Cal. Tech. group working with similar powder had noticed that exposure of the powder to sunlight, which darkened it, greatly improved the ballistic properties.

An immediate solution of the problem was achieved by introducing small amounts of darkening agents such as carbon black and black dyes, which eliminated the erratic behavior observed with light powders. Later the use of darkening agents for this purpose became standard practice.

This experience showed that there was a great need for fundamental research on propellants, and resulted in the establishment of an extensive propellant-research program by Section H. Contracts were set up with the University of Wisconsin and the University of Minnesota for studying the fundamental properties of propellants in relation to their use in rockets. This research, together with that carried on at Indian Head, later showed that the "termite" fissuring was due to radiation from the very hot luminous gases thrown off by the burning powders. This radiation, when allowed to penetrate the propellant grain, was absorbed by the inevitable microscopic specks of dirt in the propellant and caused local heating which actually made the solid propellant burst into flame internally.

This research not only discovered the fundamental cause of a very serious problem, but enabled the effect to be put to good use, as in later problems the "radiation effect," carefully controlled, was used to promote smooth burning and better functioning of rocket propellants.

Using a propellant known as JPT,[5] containing some diphenylamine as well as ethyl centralite, the 4.5-inch rocket was standardized under the designation M8 and put into quantity production. However, improvements in design continued and resulting modifications were put into production in a succession of improved models. The first effort to improve the M8 was directed toward the development of a stronger motor tube to make

[4] The function of a stabilizer is to prevent accumulation of oxides of nitrogen formed in nitrocellulose-powder deterioration as these oxides catalyze the decomposition and can cause spontaneous explosions.

[5] JPT — Jet Propulsion Tubular.

the rocket completely safe under conditions likely to be encountered in service. This work was begun by the NDRC under a contract with Budd Induction Heating, Inc. With their co-operation, the T41 model was developed. It was similar to the M8 in dimensions but was engineered to have a strength approximately twice that of the latter. This model was never put into quantity production but experience gained in its development was used by the Ordnance Department in improving the standard round. The second problem to be attacked was how to improve the behavior of the propellant at low temperatures. The standard charge burned well down to a temperature of 20° F., but below this temperature the rocket would extinguish suddenly in the middle of burning and then continue to burn in a series of intermittent chuffs. The search for a solution to this problem led to the study of the effect of powder composition on burning properties. The final solution was surprisingly simple. It consisted of the addition of a small percentage of potassium sulfate to the powder, which gave a powder behaving well in the 4.5-inch rocket down to a temperature of 30° below zero Fahrenheit.

Improvements in propellant and rocket design resulted in a new model of the M8, designated the M8A3. This model was identical with the M8 except for the strengthening of the motor at the threads near the head end and for the use of a "mixed charge" in which one third of the powder sticks contained potassium sulfate salt. Soon afterwards the production of the M8 was discontinued, and a new improved model put into large production designated the T22. This was patterned closely on the T41 design developed by the Budd Company for NDRC and used a charge of the modified powder. The T22 was superior to the M8 in safety, accuracy, and reliability at extreme temperatures.

The 4.5-inch rocket had never exhibited as great an accuracy as had been expected when the rocket was first designed and produced. Part of this difficulty was due to poor ignition, which characterized the early models and caused the rocket to leave the launcher with a low velocity, and part was due to the small size of the fins, which gave the rocket adequate stability but did not suffice to counteract accidental yaw produced during burning. Furthermore, the Navy and the California Institute of Technology had developed a zero-length post launcher which had many advantages over the bulky tube launchers previously used on aircraft. For these reasons, GWU design and experimental workers, with the co-operation of the Budd Wheel Company, developed a fixed-fin assembly which could be attached to a standard 4.5-inch rocket in the field. In addition, an improved "bayonet" igniter was developed by the GWU and Bell Telephone Laboratories which gave rapid and reliable ignition at all service temperatures. The T22 rocket with the new fins and igniter could be launched from an adapter attached to the zero-length post launchers, and

flew with excellent accuracy. These improvements were incorporated in a field kit and produced in quantity by the Ordnance Department for shipment to the European and Pacific theaters, where stores of T22 and M8 rockets were on hand.

Thus a relatively lightweight weapon was developed — the entire rocket weighs less than 40 pounds — which would permit an airplane to shoot with accuracy a projectile carrying over 4 pounds of TNT. These were first fired against the enemy in the winter of 1943–44 from cluster launchers on AAF planes in attacks on Japanese ground installations in Burma.

Using launchers designed by the Ordnance Department, the Army ground forces also made some use of these rockets, both in Europe and in the Pacific islands. A drenching fire on an area target could be laid down with rockets, and with jettisonable launchers a tank could provide its own artillery barrage.

By the fall of 1943 the aircraft rocket had become established as a potent offensive weapon against medium ground installations, such as vehicles, trains, and ammunition dumps, and against small boats and submarines. Aircraft rockets were being used extensively by the Russians and the British, and increasingly by the U.S. Navy. The Navy 3.5-inch rocket, modeled after the British UP3, had proved very effective against Japanese barges in the Pacific, and against German submarines in the Atlantic.

The Air Forces had used the 4.5-inch M8 rocket to a limited extent in the Pacific, Africa, and Italy. It became apparent that while the M8 rocket was an effective air-to-ground weapon, it was not sufficiently powerful to defeat many of the targets encountered by fighter pilots. Moreover, its inaccuracy limited its use to ranges of about 500 yards, which is too close if the target is defended by antiaircraft fire. In view of these limitations, the Air Forces requested the Ordnance Department in December 1943 to develop a "Super" 4.5-inch rocket, to be more accurate than, and twice as powerful as, the M8. This weapon was to be effective against lightly armored surface or underwater targets, such as destroyers or submarines, as well as against troop concentrations and ammunition dumps. It was further requested that the development be carried on highest priority, to produce 1000 rounds for test by August 1, 1944.

In March 1944, the Ordnance Department asked the NDRC to take responsibility for the complete development of this weapon. This combined earlier informal requests and the project was formally accepted in April, and assigned to Division 3, Section H. Although it was clear at that time that the August 1 deadline could not be met, the development was undertaken under highest priority, with Gibson, Kossiakoff, and Alec Glenn, Chief Engineer at ABL, taking the main responsibility, with expectations of completion by late fall.

Even before the Air Force's request was made the Cal. Tech. group of

Division 3 had designed for the Navy a powerful aircraft rocket, called the 5-inch HVAR (high-velocity aircraft rocket) and termed appropriately Holy Moses. This rocket had a velocity of 1300 feet per second and carried a 50-pound head with 7 pounds of explosive. It was propelled by a single grain of dry-extruded double-base powder, identical in composition with that used in all Navy rockets. While this rocket, if completed successfully, would satisfy all the requirements of the Air Forces, it depended on a propellant which had never been produced on a large scale. Its supply was very critical, and would remain so for at least a year, in view of the heavy machinery required for its manufacture. Since the need for a powerful aircraft rocket was manifest, it appeared necessary to develop an alternate weapon to augment the first, or to replace it if the difficulties in supply or performance proved insuperable.

In designing the Super 4.5-inch rocket, the most important point was the choice of propellant. The only propellant in quantity production at that time was JPT (Jet Propulsion Tubular) powder — a short-burning double-base powder manufactured by the solvent process for the bazooka and the 4.5-inch M8 rockets. It was therefore decided to use a powder which could be produced by the same equipment and methods as JPT.

Powder made by the solvent process, as discussed in Chapter V, must be dried after extrusion, which imposes a limitation on the thickness of grains. Thin-webbed grains are well suited to low-velocity, short-burning rockets like the bazooka, but not so well to large high-velocity rockets. The 4.5-inch M8 rocket, previously discussed, represented the most satisfactory rocket of this caliber which it had been possible to make with JPT powder.

Two courses suggested themselves. One was to use existing powder in a rocket of an unorthodox design to achieve the high density of loading required. The other was to modify the composition and granulation of JPT to meet the ballistic requirement of the new rocket. The first of these had already been exploited in the 4.5-inch "step" motor, which vented the gases through peripheral jets located along the length of the motor. However, early tests showed that the step motor would not meet the requirements for the new rocket on two counts. First, severe blast from the peripheral jets would greatly complicate launching of the rocket from aircraft. Second, the increase in the density of loading achieved was insufficient to attain the required velocity with an adequate pay load. There remained, therefore, the necessity of developing quickly a modified solvent powder, different in composition and granulation from any propellant previously made.

Fortunately, work on the burning properties of experimental powders at Indian Head had established an extensive body of knowledge on the relation between powder composition and rate of burning. This information made it possible to predict very closely just what constituents must be

added to JPT, and in what proportions, in order to achieve the desired ballistic properties. Aided by the experience of the Hercules Powder Company in the manufacture of rocket powder, a new propellant was developed completely in a period of two months. It turned out not only to have the correct burning and physical properties, but to be markedly superior to JPT in its behavior at extreme temperatures. This powder was standardized in July and ordered in quantity for extensive tests.

With the solution to the propellant problem in hand, one further problem remained. This was the design of a rocket motor which would be at the same time light, strong, accurately formed, and yet readily and cheaply manufactured. Up to this time an experimental motor had been used, which had all of the necessary characteristics save ease of manufacture. Its use permitted the completion of design and establishment of the performance characteristics of the rocket with respect to behavior at extreme temperatures and accuracy when fired from aircraft. The Service requirements in both respects were found to have been achieved.

The development of a production-type rocket motor at first was handicapped by the high-priority artillery-ammunition production programs which saturated manufacturers of steel products. After several unsuccessful experiences with large manufacturers, contact was established in July with the Hydril Corporation, manufacturers of oilwell casing. This concern, in co-operation with the Cal. Tech. group, had developed a process for forming a nozzle from a steel tube with the great precision required to assure exact alignment of the nozzle with the rocket body. By September, the method had been successfully applied to forming commercial 4.5-inch steel tubing, and by November motors from alloy-steel tubing with a nozzle formed directly on the motor tube were made and tested. This method of manufacture made it possible to produce a rocket motor for the Super 4.5-inch rocket with all of the desired properties more quickly and easily than had been possible by other methods for other rockets of similar size.

The design of heads for the new rocket was not a difficult one, owing to the experience previously gained by NDRC and the Services. Two heads were designed to satisfy the AAF requirements — a fragmentation head with an instantaneous nose fuze for general purpose use, and a semi-armor-piercing head with delay base fuze for use against lightly armored surface and underwater targets. The shape of the latter was scaled up directly from the underwater head designed by the Cal. Tech. group for the 3.5-inch rocket, which had proved to have excellent water entry and underwater drag characteristics.

The complete round was fully standardized in December 1944 — eight months after its development had been authorized. It was 6 feet long, weighed 103 pounds, 40 pounds of which was pay load, and had a velocity of about 1300 feet per second when launched from aircraft. It was stabilized

by four fixed fins, and equipped with launching lugs for attachment to zero-length post launchers.

By the time the Super 4.5-inch rocket was fully developed and found to meet AAF specifications, the 5-inch HVAR had been successfully developed and accepted by the AAF and the Navy. Facilities for making dry-extruded powder were being expanded, with the expectation of meeting requirements by the summer of 1945, and production of metal components had begun. However, the Service requirements for other rockets, particularly for those needing dry-extruded powder, were increasing at a tremendous rate due to the successful use of rockets in all theaters. Therefore, the Army Air Forces requested in November that the Super 4.5-inch rocket be put into production to assure beyond question the supply of powerful aircraft rockets.

Unfortunately, the request came at a time when the shell- and bomb-production programs had been put on an emergency expansion basis. Consequently, the program was reduced to a pilot production of 10,000 rounds, with a request to set up facilities for a later expansion, if required. In December, the final design was completed by NDRC and turned over to the Ordnance Department for production.

It turned out in the spring of 1945 that the production of 5-inch HVAR would meet the Service requirements. However, the Super 4.5-inch rocket had characteristics which made its use advantageous under certain circumstances. It was largely flashless and was better adapted to use with certain types of proximity fuzes than the HVAR. These characteristics made it excellently suited for use against Japanese suicide planes, particularly for night fighters. Just before V–J Day the Navy became greatly interested in obtaining this rocket in quantity, but the war ended before the round could be put into use.

# CHAPTER XI

## *RECOILLESS GUNS*

THE STORY of the recoilless gun really begins with a different development, that of the 4.2-inch chemical mortar, for which Section H made an important improvement. In 1942 the Indian Head group had been engaged in developing rockets for carrying chemicals and incendiary bombs for the Chemical Warfare Service, and W. H. Kayser (later Colonel) of CWS was making frequent visits to Indian Head. On seeing the facilities there for measuring the pressure within the rocket motor, he asked permission to bring down a 4.2-inch chemical mortar in order to obtain pressure-time curves for various charges. The 4.2 mortar was set up just outside the Bomb Proof and the shells were fired into the Potomac River. A strain-gauge pressure unit, designed for use with rockets, was screwed into the breech of the mortar and pressure-time curves obtained for many different charges. These pressure-time curves indicated that the peak pressure was reached before the shell had traveled more than 4 or 5 inches. It was therefore concluded that it would be safe to fire the mortar at higher pressures because this portion of the gun was considerably stronger. Increasing the operating pressure of the gun permitted an increase in range of from 2200 to 3300 yards. At the same time, the pressure-time curves suggested that the type of powder being used was not well suited and that a thicker web would, in all probability, result in materially increasing the range without increasing the peak pressure. Kayser was very much interested in this suggestion and wanted to know if the Indian Head Group would undertake the development of a suitable charge, which it agreed to do, provided an officer could be detailed to supervise test firings.

Further tests were made and pressure-travel curves obtained by attaching a long stick to the nose of the shell so that the exit of the shell could be photographed with a high-speed camera and this motion synchronized and related to the pressure-time curve.

Lieutenant A. R. T. Denues went to Indian Head to take charge of this work, and under his guidance it went forward with great rapidity; in a relatively short period of time a charge of powder for the 4.2-inch chemical mortar had been developed and standardized which increased the range from 3300 yards to 4500 yards. The chief difference between this so-called Indian Head charge and the standard charge was in the web thickness of

the powder used. The maximum pressure remained about the same but the longer duration of the burning increased the velocity to a point where the range of 4500 yards[1] was attained. This success for Colonel Kayser had its big pay-off a year later.

Late one Saturday afternoon Hickman was sitting in his office with Mr. Pope of the Budd Wheel Company when he received a telephone call from Colonel Kayser. There was an urgent tactical need to modify the chemical mortar for use in direct fire. The Colonel recalled some previous discussions about the rocket principle and wondered if the chemical mortar could be modified in this way to get direct fire. He was pleased when assured that modification to a recoilless gun would do the trick.

There are now several American recoilless guns, some developed by the NDRC and others by the Ordnance Department. We cannot trace here the history of all of these, and we shall discuss only the 4.2-inch recoilless chemical mortar.

A recoilless gun is in a way a cross between a gun and a rocket. If well designed, it retains the chief advantages of each. (There are times when the workers felt that some experimental models retained the disadvantages of each.) We can think of a recoilless gun as being essentially a standard gun with a rocket butted against the rear end of the breech. On firing, both the regular gun and the attached rocket shoot simultaneously. The gun shoots a projectile forward and suffers a backward recoil. At the same time, the rocket shoots gases backward and supplies a forward recoil. These two forces balance each other — if the recoilless gun is well designed — and there is no resultant force applied in either direction. Thus, we have a truly recoilless gun. In practice there is no reason for the gun and the rocket portions to be separated. The wall between them is eliminated, and the gun and rocket share the same chamber and the same powder charge. Thus we have in a recoilless gun, a standard gun barrel with the ordinary powder chamber and the breech block replaced by a somewhat different powder chamber which continues to the rear and there forms an open nozzle.

As compared to a standard gun, the recoilless gun has the advantage that there is no need for a heavy and elaborate recoil-absorbing mechanism. This mechanism makes up a large part of the weight of an ordinary gun. Hence the recoilless gun provides the advantage of lightness. There is no need for recoil absorbers since no recoil forces need be taken care of and the mounting may be made very light. The recoilless gun has one disadvantage as compared to the ordinary gun, a backward blast of hot gas. This is not too serious; the gunner need only stand to the side of his gun.

As compared to a rocket, the chief advantage of the recoilless gun is its much greater accuracy. The accuracy approaches that of an ordinary gun.

[1]Section H later made additional modifications which resulted in a range of 6000 yards.

Unlike most rockets the projectile is guided by the barrel during its entire period of thrust application, and the rifling gives it the high accuracy of spin stabilization. On the whole, the recoilless gun provides a lightweight weapon which is mobile and flexible, and at the same time has the high accuracy achieved only by guns.

The recoilless chemical mortar is really not a mortar at all. It could scarcely be a true mortar and be suitable for direct fire. For a mortar is essentially a type of gun which in use is aimed upward at an angle of 45° or more, its muzzle pointing to the sky, and its base resting against the earth, which absorbs the recoil. In this way, there is no need for a recoil-absorbing mechanism — the earth is always available. Therefore, the mortar, with its much simpler mount, is a highly mobile type of howitzer.

The standard 4.2-inch chemical mortar is so called because it was originally developed by the Chemical Warfare Service for the projection of shells containing smoke, gas, and the like. However, it was found that this mortar was able to project a comparatively heavy round. When this high capacity was used, for high explosive, the chemical mortar was found effective in shooting a very powerful shell. It was also a very mobile weapon, as guns go, and because of its high mobility and the high striking power of its HE round, this mortar became one of the outstanding weapons of the present war. It was particularly valuable for use in rough terrain, such as our troops encountered in the mountains of Italy or when fighting in the jungles of the South Pacific islands. In such locations there was no way of bringing up any other weapon with a comparable striking power.

The chemical mortar is essentially a muzzle-loading weapon. In use, it is set up with its base plate resting against the ground, and the muzzle pointing upward, the barrel being supported by a simple standard. The shell and powder charge come as a single unit. Whoever fires the weapon simply drops this unit into the open muzzle. The powder charge carries a percussion firing element. A firing pin is located at the bottom of the mortar barrel. When the shell strikes the bottom of the barrel, this percussion element strikes the firing pin, and the propellant is ignited. The chemical mortar, unlike most mortars, is rifled. The shell is equipped with a brass rotating disk small enough to allow the shell to slide down the barrel. Upon firing, the pressure of the propellent gases forces the rotating disk to expand, so that it fills the barrel and engages the riflings. This imparts spin to the shell and increases the accuracy and efficiency of the mortar.

It will be seen that the chemical mortar, or any mortar, lobs its shells through a high angle and drops them on top of the enemy, instead of shooting directly along a relatively flat trajectory as does the ordinary gun. This renders the mortar valuable in many tactical situations, such as when the enemy position is located behind a hill or otherwise screened from direct fire. The mortar is also useful in laying down a fairly heavy barrage over a

large area. But for direct fire against point targets, the mortar is scarcely the ideal weapon. There was a top-priority tactical need for a highly mobile direct-fire weapon of high accuracy, shooting a shell of high-explosive capacity to be used against point targets such as Japanese bunkers.

It was with this tactical need and with facts concerning the mortar in mind that Colonel Kayser approached Section H in the hope that the application of the rocket principle might in some way enable the chemical mortar to be modified so that it could be used for direct fire, and would still be free from the necessity of a heavy and cumbersome recoil-absorbing mechanism and mounts. The recoilless modification made it more a gun and less a mortar, but it did enable its use in direct fire without sacrificing the high mobility of the weapon.

The first experimental model, which was constructed by the Budd Wheel Company within a week, was demonstrated by Section H at Edgewood Arsenal. It was in all essentials similar to the final weapon which proved successful beyond everyone's expectation. Of course, the first model was not perfected to the point where it could be taken immediately into the field and used by troops. The entire development of the recoilless mortar to such perfection took almost a year — which may seem a long time, but was actually short as such things go.

The first model of the recoilless chemical mortar, like the final model, had a centrally located single nozzle at the back of the chamber. The barrel was simply that of a standard chemical mortar shortened and rethreaded at the breech. Inside the expansion part of the nozzle there were canted vanes which through the action of the blast of gases imparted a rotational torque to the gun, thus counteracting that caused by the rotation of the shell in the barrel. The shell was made to rotate by the use of riflings, as in practically all guns. These canted vanes were found to be a feature worth retaining in the final Service model.

In the first model, as in the final Service model, the propellant was in the form of thin disks of sheet powder. This laminated disk charge was patterned after the laminated charge used in the super-bazooka — and the super-bazooka charge was itself suggested by the laminated sheet charge of the standard chemical mortar. In the recoilless chemical mortar, the sheet-powder disks with a hole punched in the middle were stacked and placed over a tube. This central tube served the double purpose of supporting the powder and carrying the igniter. The igniter was also of special design, similar to the ingenious igniter developed for the super-bazooka.

In the first test at Edgewood Arsenal the recoilless gun was placed on a pair of ordinary sawhorses and fired. The gun recoiled slightly, falling a foot or so behind the sawhorses. Some minor adjustments were made on the throat of the nozzle, and the second round was fired successfully, the gun

remaining on the horses. Thus it was indeed, on this second shot, a recoilless gun. As sawhorses are unsatisfactory gun mountings, the gun, after this initial adjustment, was mounted on a .50-caliber machine-gun mount (certainly the first time the U.S. Army had a gun that, in position on a lightweight machine-gun mount, could fire a four-inch shell containing 8½ pounds of TNT). The gun performed satisfactorily with complete freedom from recoil. After a few rounds, the canted vanes blew out of the nozzle. This meant that the rotational force due to the rifling was no longer balanced. However, there appeared to be no undue stress on the mounting.

This first model was a "blacksmith" job, rather heavy and cumbersome. It had been built mainly to test the recoilless idea, the central nozzle, the laminated-disk propellent charge, and some of the other features. A second model was built with the chief purpose of reducing the weight of the powder chamber and nozzle. Since the first model seemed to work well without any rotational canted vanes, these vanes were omitted from model 2. When this model was first tested, the shots were successful from practically every standpoint except that the rotational forces turned out to be serious — one leg of the .50-caliber machine-gun mount was badly bent after a few rounds. All later models included the canted vanes which were originally designed and tried out in the beginning.

For several months, work was done on developing experimental models. Effort was made to obtain optimum design of a lighter-weight chamber, greater ease of manufacture, and ruggedness and flexibility. Various methods of firing were tried out. An electrical method was first used that proved not well suited for use in the field. Various percussion methods, using a trigger as well as simply dropping the shell in, were tried. Progress was steady, and ballistic performance on the whole was excellent with these experimental models, until certain firings conducted on a cold day gave the first hint that the ignition system was not suitable for use in cold weather. The temperature problem gave the workers a considerable amount of trouble.

One good feature of the experimental models was a velocity of the order of 700 feet per second — a velocity exceeding the 550 feet per second originally requested by the Chemical Warfare Service. When the CWS officers learned of the higher velocity of 700 feet per second they were immensely pleased — so pleased that it seemed worth while for them to set the goal higher. Accordingly they asked Section H to redesign the gun to give a velocity of about 1000 feet per second. This required extensive changes, since it would be necessary to have the gun work at higher pressure — up to 25,000 rather than some 12,000 pounds per square inch.

Increasing the pressure required a stronger chamber. In order to achieve a stronger chamber, and at the same time keep down the weight, a near-

miracle would be required. An engineer suggested that a model made of extremely high-yield steel might achieve this purpose, and model 5 of the recoilless chemical mortar was built of this steel, designed to withstand 25,000 pounds per square inch pressure. As a first trial, the powder charge was so chosen that a chamber pressure in the neighborhood of 14,000 pounds per square inch was expected, well within the presumed safety limit of this model. At this time, the experimental firings were using a trigger mechanism for firing the gun. A long rope or lanyard was used so that the firing men need not stand too close to the gun. On the first shot of this model 5, when the firing man pulled the lanyard, the whole gun exploded in a spectacular fashion with large pieces of steel flying in all directions. The first reaction of the firing man was amusing; he burst out, "I knew I pulled that damned thing too hard!" This, though perhaps a normal reaction at the time, was hardly an accurate explanation of the unexpected explosion of the gun. The serious question was to discover whether the powder charge had misbehaved to cause a pressure far in excess of the intended 14,000 pounds per square inch, or whether the steel chamber was at fault and had fragmented under a pressure lower than its supposed 25,000 pounds. There was, as in practically all experimental firings, a copper-ball pressure gauge attached to this experimental model. A search was immediately instituted for the copper-ball pressure gauge, which, of course, had been thrown from the gun with other fragments. The search was unsuccessful, until it was catalyzed by the offer of a fifth of Scotch to the worker who found the gauge. When found, the gauge registered only 13,800 pounds per square inch pressure. The propellant charge had behaved satisfactorily and the fault lay in the extremely high-yield steel used for the chamber. Such steel is apparently far too brittle to withstand the sudden pressures which occur in a gun. Further tests confirmed the conclusion that even with the best modern steels, it was not practical to build a simple recoilless gun with lightweight components which would give a muzzle velocity of 1000 feet per second. A heavier gun would have made possible this higher velocity. Subsequently the Chemical Warfare Service withdrew their request for a higher velocity, and it was decided to develop the lightweight gun with a velocity of some 700 feet per second. After this decision, the development was very rapid.

We have mentioned that low-temperature firing had given poor results. Rounds which were expected to leave the gun with a velocity of 700 feet per second and travel some 3800 yards misbehaved so badly at low temperatures that the range was reduced to 15 feet. The velocity was not measured but was obviously rather low. These "poop" shots, in which shells traveled 15 feet or so, continued to be encountered on cold days or in experimental low-temperature firings. A great deal of work was required to overcome

this difficulty. Many trials were made with improved systems of ignition and moderate refinements in the propellant charge. After many months of work, Section H had a recoilless gun that would fire well on even the coldest days.

The Chemical Warfare Service desired a recoilless gun having a high rate of fire. This was most easily obtained by sticking to the method of firing the ordinary mortar — that is, having the shell and propellant charge as a single unit which would fire when it was simply dropped into the mortar. As a result, trigger-firing mechanisms and the like were abandoned. Firing was achieved by a percussion element in the cartridge case, and a firing pin in the powder chamber, quite similar to the mechanism used in the standard chemical mortar. The question now arose as to how low-angle fire was to be achieved. A shell obviously will not drop into the muzzle if the barrel is horizontal or close to it.

The problem of slamming the shell against the firing pin, even when the mortar was horizontal, was solved by an ingenious idea called a "driver rocket." This small driver rocket, weighing less than a pound, was fastened to the front of the shell in such a manner that, upon firing, it would propel the shell backwards. Thus to fire a round when the mortar barrel is horizontal or nearly so, the soldier has only to slip the shell into the muzzle end of the mortar and pull the firing ring on the driver rocket. The action of the driver rocket slams the shell back against the firing pin in the base of the mortar barrel and thus fires the main charge. The first driver rocket that was fired worked like a charm as far as driving the shell back against the firing pin was concerned.

One problem concerning the driver rocket remained to be solved. After firing, it stuck on the front end of the shell ahead of the fuze. It was necessary that it be firmly attached to the shell so that it could be handled and so that it would drive the shell down into the firing pin. It was also necessary that it should fall off the shell upon firing, leaving the fuze clear and ready to function. To remedy this apparently small defect took a great deal of work. The first really satisfactory driver rocket was ready for use at a demonstration of the recoilless gun scheduled at Edgewood Arsenal before high officers of the Chemical Warfare Service. It was recognized that the weapon, though not quite complete, was sufficiently well developed for demonstration.

On the previous evening, the rocket workers from Section H were at Edgewood for a short rehearsal, the principal purpose of which was to establish some preliminary range data at the site of the demonstration. The rocket workers in their experimental firings had used only inert heads, shells which contained no high-explosive or other chemicals, but which were filled with sand. For the Edgewood demonstration, there were two varieties

of shells available, some filled with high explosive and others, called FS[2] shells, filled with a liquid which produced smoke upon impact. The NDRC workers naturally decided to use the smoke shells for their rehearsal and for their demonstration next day. The first FS shell was picked out in routine fashion, but the routine ended with the firing of this round. For, when the round was fired, the recoilless gun jumped into the air, turned over, and landed a few feet ahead of its original position. The shell burst only some thirty feet in front of the gun because of premature functioning of the fuze.

How fortunate that this was only a dress rehearsal! The NDRC workers hesitated to call off the demonstration, at a time when the recoilless gun had actually been coming along so well. With less than eighteen hours to go before the demonstration, the workers retired to Baltimore for a conference on the possible cause of the mishap. The only change from the previous satisfactory firings was the use of the liquid-filled shell. It was decided that something must be wrong with the shell. A possible explanation of the misbehavior came from the fact that the shell was only partially filled, an empty space being left for expansion of the liquid on hot days. This space had allowed the liquid to be shaken around inside the shell. Thus when the driver rocket slammed the shell to the back of the barrel, the liquid had moved toward the nose of the shell. When the shell was then shot forward, the liquid had slammed back against the base of the shell casing, causing this casing to bulge against the mortar and thus tend to stick in the barrel. This hypothesis seems entirely correct. Solid-filled shells, containing TNT or *solid* smoke producers, are used successfully in flat fire with the recoilless chemical mortar.

The NDRC workers had only their plausible hypothesis to go on. They went back to Edgewood and spent the entire morning of the day of the test taking the liquid out of these FS shells, and replacing it with sand. Sand-filled shells had always worked well. There was no time to test the hypothesis that the cause of misbehavior lay in the liquid-filled shells, and this seemed the only way to carry on the demonstration. Thus, at noontime, the chief scientists on this project from the NDRC lunched, apparently unperturbed, with the CWS officers who were to witness the demonstration. Meanwhile the crew of NDRC worked madly to get the gun in position and set up for the demonstration. The demonstration was staged promptly and successfully at the scheduled time — with no rehearsal.

The model which was fired at this demonstration was almost identical with the final form. About two months were spent in completing the solution to the problem of firing at low temperatures, and in putting in the last refinements on the mechanical construction of the recoilless mortar and

[2] FS is the typically cryptic designation meaning "fuming stuff," chlorosulphonic acid mixture.

its mount and ammunition. By the end of October 1944, the final model was completely fixed.[3] Twenty-five guns were ordered by Section H from Budd for Service testing by CWS. These, complete with some five thousand rounds of ammunition, were finished and ready, and the Service tests were held about six weeks later. Early in 1945, Service teams, equipped with this latest and highly effective recoilless chemical mortar, went out into the field.

[3]The principal workers engaged in this project were Major A. R. T. Denues, and other officers, Dr. R. B. Kershner, J. M. Woods, Lieutenant Donovan (USNR), O. R. Roderick, and G. H. Hoppin on procurement.

# CHAPTER XII

## *FLAME THROWERS AND ROBOT BOMBS*

EARLY in 1944 the Chemical Warfare Service requested the assistance of Section H in developing rocket-propellant pressurizing elements for flame throwers. These elements consisted of a small chamber holding a charge of powder which, on being ignited, would supply the gas pressure required to push a piston ejecting the fuel. The first proposal was for a unit to be operated from aircraft. The cylinder of compressed nitrogen ordinarily used to provide pressure to eject the fuel required too much space and weight to be practical. To answer this objection the rocket-pressurized airborne flame thrower was developed.

This pressure generator unit consisted of sixteen rocket motors, each containing two standard 5-inch grains of ⅞-inch rocket powder, which were ripple fired at intervals controlled by a step switch and small electric motor. In addition attached to the fuel tank there was a hydrostat, which, through a microswitch, interrupted the firing if the pressure in the tank rose too high.

Preliminary ground and air firings in March 1944 showed the practicability of the pressurizing unit, but as, in use, the flame-thrower unit demonstrated no advantage over dropping incendiary bombs, the project was not continued.

Another CWS–Section H development was a one-shot flame thrower, usually referred to as the OSFT. The Chemical Warfare Service had developed a weapon, very light in weight, which the soldier could use to project a single searing outburst of burning fuel some fifty yards. Though it held only the one shot, its light weight made up for its small load. Its cylindrical fuel tank held two gallons of thickened gasoline. A coil of tubing, wrapped around the fuel tank and connected with it by a valve at the rear of the tank, held compressed carbon dioxide. The tank contained a light piston. When the valve was opened, carbon dioxide escaped from the coil and pushed against the piston, which in turn exerted pressure on the fuel. Under this pressure, the fuel would break through a thin disk of metal, and stream out of a nozzle at the front of the fuel tank. The rupture of the thin disk also operated an ignition mechanism which set fire to the gasoline stream.

The use of liquefied carbon dioxide to generate the necessary pressure had some disadvantages. One was that it would be hard to supply compressed

carbon dioxide in the field, for charging the flame throwers. The second was that the carbon dioxide would be under pressure while the soldier was carrying his weapon. This made it necessary to construct the flame thrower with absolutely gas-tight joints, not an easy matter. The NDRC was asked to develop a "powder pressurizer" that would eliminate the drawbacks of the carbon dioxide pressure system, and would also reduce the total weight of the flame thrower.

Specifically, the Chemical Warfare Service stated that the powder-pressurizer unit should develop a pressure of 350 pounds per square inch and keep this pressure on the fuel for some 3 seconds. This called for a special propellant, and the Section H scientists used one of their modified rocket propellants. The powder grain, weighing less than a quarter of a pound, was a solid cylinder very much like the grains used in the JATO unit to be discussed later; the sides of the powder cylinder were restricted, so that only the end surfaces burned. The powder burned in a little cylinder 1.6 inches in diameter and 1.2 inches in length. It generated a pressure of some 1000 pounds per square inch in the little cylinder, and this pressure was reduced through a nozzle, much like that of a conventional rocket motor, to give the specified value of 350 pounds per square inch on the piston which pushed the fuel. When the completed unit was tested, it did exactly what it was supposed to do, but it did not cause the flame thrower to work properly. Investigation showed that the original requirements were wrong; that the pressure of 350 pounds per square inch, which was right for the standard flame throwers, was wrong for the portable ones. The Section H workers, co-operating with Division 11 of the NDRC, carried out tests and found the desirable pressure in the OSFT to be some 200 pounds per square inch. Accordingly they redesigned their pressure unit, at the same time improving other components of the flame thrower.

Some work still needed to be done on the flame thrower itself, apart from the pressurizer. In March 1945, the OSFT came back to the Section H workers with the request that they develop the complete unit,[1] from pressurizer to nozzle. By April 5, the OSFT was sufficiently well developed for a semiproduction model to be demonstrated at Edgewood Arsenal. Representatives of the Army Ground Forces were impressed by the weapon, and further tests were made by the Infantry Board in the end of April.

The Infantry Board reported favorably on the OSFT, also recommending several modifications which would improve the usefulness of the weapon in the field. They recommended that 1500 units be supplied immediately to the Services. The recommended modifications, including an increase in the fuel supply, and many minor improvements were developed during the next two months. The final production design was finished on July 25.

[1]This work was carried on in co-operation with Division 11, NDRC, which had been engaged in earlier flame-thrower developments.

The weapon, weighing less than 40 pounds, would enable the infantryman to shoot 3 gallons of flaming gasoline nearly 50 yards — a 7-second blast of concentrated flame.

This was one highly specialized application, quite impracticable with ordinary gun propellants in use before the war, which was quickly developed by the rocket group using the specialized rocket charges which they had made possible. Another application helped the American version of one of the devastating weapons which the Germans used against Britain, the V-1. This robot bomb, powered by a well-designed jet-propulsion engine, was a most effective weapon in the tactical use for which the Germans built it. After some of these bombs had been captured, it did not take long for our Army to produce an American version. The American version, known as the JB-2, was available in fair quantity by the winter of 1944. It had certain possible applications in the war against Japan, and tests were proceeding so that the weapon would be ready for use when a tactical application arose.

But there was some trouble with the launcher. The robot bomb had to be launched at a speed exceeding 200 miles per hour. Because of the delicate guiding instruments in the bomb, this speed had to be built up relatively slowly; the bomb could not be launched from a gun or similar catapult. The German launcher consisted basically of a ramp carrying a tube 160 feet long. A piston was propelled the length of this tube by expanding gases, somewhat in the manner of a gun, but using much lower pressures in order to avoid too great an acceleration to the robot bomb. The piston carried a hook which projected through a slit running the full length of the tube, and the robot bomb was carried along by this hook. An essential feature of the launcher was a soft-metal sealing strip, used to prevent leakage of the propelling gases through the slit in the tube; the sealing strip was flexible enough to pass around the hook as it progressed through the launcher. The Germans generated the propelling gases for their launcher by treating concentrated hydrogen peroxide with permanganate. Although this is an excellent reaction for gas production, it had two disadvantages as far as our use of it was concerned. In the first place, concentrated hydrogen peroxide is an inconvenient material to handle. In the second place, large-scale production of concentrated hydrogen peroxide had not been carried on in the United States. It therefore seemed likely to our Army that some more practical way[2] of generating propelling gases for the JB-2 launcher could be found.

It was this question of providing a simpler way of generating propelling gases which came to the Section H group in the winter of 1944. The

[2]AAF experimented with launching by steam and by rocket, using Division 8 propellants. CWS undertook development of hydrogen peroxide production and launching systems.

problem was solved with the aid of one of the special rocket propellants developed by Section H — the same propellant used in the 4.5-inch aircraft rocket discussed in Chapter X. Seven "cartridges," each containing an appropriate amount of propellant, were placed along the 160-foot tube at the proper spacings. As the first cartridge was fired, its propellent gases fed into the launcher tube, and started the piston on its way. As the piston passed the second cartridge, the gas pressure behind the piston actuated a small percussion element in the second cartridge, and ignited its propellant. The propellent gases from the second cartridge then sustained the force on the piston in the launcher tube, and caused it to move still faster. Similarly, each successive cartridge fired as the piston passed beyond it, and each contributed its push.

When this idea was submitted to the Army, the Section H group was requested to demonstrate it in a launcher of full length, but of smaller diameter than the one to be actually used. The scale model that was constructed did not, of course, launch a robot bomb. It only projected a piston of the appropriate weight. But it did show that the proposed method of launching the bomb was entirely sound.

After this successful demonstration of the principle, the go-ahead signal was given for the building of a full-scale launcher, with a tube nearly a foot in diameter. This full-scale model was built at Eglin Field, Florida. Test firings using dummy rounds revealed the need for slight improvements in some of the mechanical parts of the launcher. With these modifications, the first launching of an actual robot bomb was attempted. The multiple-cartridge system worked well, but the piston hook holding the robot bomb broke. Stronger piston hooks were obtained, after which the first successful launching[3] took place.

[3]Later Section H engineers assisted the Navy in launching JB-2 missiles at Point Magu, California.

# CHAPTER XIII

## *PUSHER ROCKETS*

MANY of the most interesting applications of rocket motors were made in gadgets in which the motor merely supplied a push. In this chapter, we shall tell of several devices using rocket motors, which were developed for the use of the Army Corps of Engineers.

An interesting pusher device using the reaction principle, although hardly to be called a rocket, was the "Donnerkeil"—a device which enables the engineers to dig holes for telephone poles very quickly. It consists of a steel rod nearly 6 feet long, with a 1-foot propellant chamber at one end, nearly 4 inches in diameter. On firing, the recoil force drives the rod into the ground. This gives a good start for a posthole; the hole, slightly over an inch in diameter, left when the rod is pulled out, is easily expanded by an explosive charge so that it will accommodate the telephone pole. Thus the device is sometimes called a portable posthole.

The Army had captured such a German device, and in December 1943, through the Ordnance Department, requested the Section H group to study it, and develop a similar one for the American Engineers. Intelligence reports indicated that the Germans used ordinary black powder for their propellant. But the NDRC workers, in experiments held in December 1943 and January 1944, found that black powder was not the best propellant to use. They chose a different propellant; actually they found that one of the standard American gun propellants could be used efficiently for this device. To get the same depth of hole which a 3-pound charge of black powder would give, they needed only ⅔ of a pound of the smokeless powder.

Further tests made in February resulted in various minor improvements, and in one rather important one. The original method for igniting the propellant was rather unsatisfactory. This ignition problem was quickly solved by using an igniter which had been first developed for the 4.5-inch rocket. This brought the Donnerkeil practically to its final form. To use the device, the soldier inserted the pointed end of the rod into the ground, loaded the chamber with its propellant charge including the ignition device, and threw a load of earth or sand on top of the propellant. When the device was fired, the dirt load "projectile" was driven upward and the recoil drove the rod deeply into the ground. The final form was demonstrated early in April, and Army observers expressed their satisfaction with their new portable postholes.

Rocket motors were also developed for a number of mine-clearing devices. The land mine, laid in fields containing many hundreds, was one of the war's greatest hindrances to the advance of infantry. One of the quickest ways of clearing a path through a minefield is to detonate high explosive along the desired path. The shock of the detonation sets off mines in the immediate vicinity. The only difficulty is that of first placing the explosive charge along the desired path through the field of live mines. There were several developments by the Engineer Corps in which rocket motors were used to propel or tow the explosive charge across the minefield so that subsequent detonation of the explosive would clear this path. The NDRC rocket workers developed the rocket motors for these devices.

The first of these devices was the Infantry Snake, which was built in the form of an attenuated ski about 100 feet long. The body was made up of overlapping metal plates, each 5 feet long and 5 inches wide. Two layers of the plates were used, and the high-explosive charge, consisting of cartridges of TNT in the final lot, were clamped between the two layers of plates in cavities provided for it. The head of the Snake carried a rocket motor, and the tail a trip mechanism. The ski shape allowed the Snake to travel over obstacles without too much deflection from its desired path. In operation, the Snake was to be set up and dragged by the soldier to the border of the minefield. Then the rocket motor would be fired, pulling the Snake into the minefield, where a trip mechanism would detonate the charge and the explosion would set off mines.

The rocket group immediately began the design of a suitable rocket motor. There were several questions which could be decided only by actual trial with the complete Snake — questions as to the magnitude of the force necessary to propel the Snake, and the like. After the Engineer Corps had made a few experimental models of the Snake available to the rocket workers, tests were made with the rocket motors which had been developed. These tests indicated several improvements which were needed, but even at that time the rocket motor would propel the Snake satisfactorily, even up a 20 per cent grade. Moreover, the Snake traveled over rough terrain without trouble, jumping ditches and clearing other obstacles.

The first field trial using the high explosive was held at the Engineer Board Field Station, Port Royal, Virginia. A minefield was laid, using German-type Schu mines, and the Snake was fired into it in the manner indicated. When the smoke cleared away, a path 95 feet long and 15 feet wide had been swept down the middle of the minefield.

Further improvements were made in the rocket motor, notably an improvement in its behavior at low and high temperatures, which was effected by changing the propellant. Also the Engineer Corps made further improvements in the high-explosive charge. In the middle of October 1944 a demonstration before the Infantry Board met with a favorable reception.

Other devices to clear minefields were also developed by the early months of 1945. One of these was the Detonating Cable Kit. This included a cable containing high explosive, and a rocket motor developed by the Section H workers. In use, the cable was projected by the aid of the rocket motor so that it lay across the minefield. Then the explosive cable was set off, clearing a path through the mines. A U-shaped rocket motor was developed for this kit, consisting of two motor chambers connected at the front end, each chamber having its own nozzle at the rear. The igniter was located in the head of the "U."

Another development involved the use of Bangalore torpedoes. This had the advantage that quite a large number of these charges were already in the field. A Bangalore torpedo is essentially a piece of pipe, perhaps 5 feet long and 2 inches in diameter, filled with high explosive. It had been supplied to the Army for use against barbed-wire entanglements, which were not actually encountered to a great extent in this war. But the blast from a Bangalore torpedo will set off some mines as well as it will blow up barbed wire. For mine clearance, the engineers proposed a string of Bangalore torpedoes, 100 feet long, to be pulled out into the minefield by a rocket motor, and then detonated, somewhat like the Infantry Snake. With Bangalore torpedoes already available in the field, only the rocket motors and the trip mechanism for detonating the explosive charge would have to be supplied. A considerable pulling force was needed for this device, and the Section H workers had to develop a special rocket motor, using one of their newly developed special propellants. The motor was itself unusual in that the nozzle was located at the front. Actually two nozzles were used, one on each side of the front of the motor, with their jets pointing back alongside of the motor chamber. This "tractor motor" had the advantage that its blast would not strike the Bangalore torpedoes. It had a great disadvantage in that it was difficult to produce in quantity.

As the European war drew to a close, and the Pacific fronts received added emphasis, an interesting change in the general picture of mine-clearing developments came about. Japanese minefields were not so sensitive as the German, and a more intense detonation was required to explode them. The Detonation Cable Kit, which cleared a very nice path through minefields laid with German Schu mines, was found to be practically ineffective in clearing fields of Japanese mines. Heavier explosive charges had to be used. The heavier mine charges called for more powerful rocket motors to propel them across the minefield, and the Section H group was again called upon to develop new motors for these more powerful mine-clearing devices.

These applications show one place where rocket motors may be generally useful. Wherever it is desired to tow or propel or push something for a short distance, rapidly but without too great a jar, with a simple, easily made pushing unit to be used once and discarded, rocket motors can be useful.

# CHAPTER XIV

## *JET-ASSISTED TAKE-OFF FOR AIRCRAFT*

THE LAUNCHING and landing of planes has always been a serious problem in the Navy's use of aircraft. Late in the fall of 1941 the Navy approached Section H with the request that the rocket principle be applied to the assisted-take-off problem. The initial idea was to use some form of catapult. Much work was done on this idea, but it soon seemed more promising to assist take-offs by a rocket motor attached directly to the plane itself.

Two purposes were involved, first to shorten the take-off runs of carrier-based planes, and second, to enable large flying boats to take off in restricted areas, in rough waters, or in an overload condition, all of which require a greater thrust than the plane propeller provides.

In March 1942, the first work by Section H on "Jet-Assisted Take-Off," JATO, was undertaken with a plan to attach rocket motors to the underside of the fuselage of the airplane. It was decided to save time by using rocket motors already available. The unit tried was composed of five British UP-3 rocket motors, each some three inches in diameter.

This unit functioned fairly well in static tests, and after some improvements and refinement, take-off tests were made in May of 1942, with rockets fired in succession. Here, too, the rocket unit functioned satisfactorily so far as assisting the plane was concerned, reducing the take-off distance from 550 feet to 135 feet. However, the blast from these rockets caused serious trouble. These JATO rocket motors were securely fastened to the plane in such a way that the blast blew over the tail surfaces all during the burning. This is in contrast to armament rockets fired from a plane, which get away after a few milliseconds; so only a very short blast hits the aircraft. This blast caused mechanical damage to the tail surfaces, and moreover the hot gases heated them severely. Some attempts to flameproof the plane surfaces were made but proved unsuccessful. British scientists working on the same problem also attempted to reduce the blast effects by firing the several rocket motors in succession. This idea was abandoned because it proved impossible at that time to make successive firing reliable.

Another difficulty with the early JATO units was that familiar bugaboo, the temperature coefficient. A JATO unit must function well and be usable without delay over a wide temperature range.

The fundamental lack of the early JATO work was a satisfactory pro-

pellant. In early 1942, the only propellant available in the U.S. in any quantity for rocket motors was ballistite, which has been discussed earlier. This was a good gun propellant; it was not an especially good rocket propellant. For the JATO unit, in particular, ballistite had a much too high temperature coefficient. Moreover, it produced high-temperature gases, so that its blast was serious. What was needed for successful JATO units was a special long-burning propellant charge which would provide a sufficiently high thrust, operate well over a wide temperature range, and produce relatively cool exhaust gases. It took a long time and a great deal of fundamental research to produce such a propellant.

The Army Air Forces had initiated research work on JATO units at the Guggenheim Aeronautical Laboratory, California Institute of Technology (GALCIT) in 1939. This research program[1] included both liquid and solid propellant types of units. In connection with the choice of a solid propellant it was soon concluded that ballistite propellants then available were difficult to apply to the solution of the JATO problem, and work was at first concentrated on pressed charges of black powder and ammonium nitrate. Units with this propellant were used on August 12, 1941, for the first successful rocket-assisted take-off of an airplane in this country. The pressed propellant charge was found to be unsatisfactory under storage conditions, and it was during this storage test program that the first promising results were obtained with a castable asphalt potassium perchlorate propellant.

JATO units utilizing this propellant were supplied to the Navy in October 1942. At this point the engineering development of larger thrust units for production quantities was undertaken by the Aerojet Engineering Corporation for both the Army and Navy. By the end of 1944, the Navy had a large number of these units in actual service use. The first Aerojet service unit had several drawbacks due to the composition of the GALCIT propellant. The propellant produced large volumes of dense smoke which coated all surfaces with which it came in contact. When the unit was used for assisted take-off from carriers, the flight deck and neighboring surfaces, including windshields of planes behind, were first obscured with dense smoke, and then covered with a fine deposit. The smoke problem, however, was not serious in connection with air-sea rescue work and other flying-boat operations.

The problem of operating JATO units over a wide ambient temperature range[2] has been a most difficult one with all propellant types.

[1]In order to determine the technical possibility of developing a long-duration, restricted-burning, solid-propellant rocket motor (which was questioned at the time), an analysis entitled "Characteristics of the Ideal Solid-Propellant Rocket Motor," dated December 1940, was made. The results of this analysis served as the fundamental starting point for the design of all types of long-duration solid-propellant rocket motors in this country.

[2]The first Aerojet units had a safe operating temperature range from 30° F. to 115° F. The improved unit as developed at the end of the war had a temperature range from 0° F. to 130° F.

Late in December of 1944, the Navy urged the NDRC scientists from the divisions working on rocket propellants to develop a better JATO unit. The problem was represented as urgent, and it was requested that an improved propellant be provided, the chief aims being to reduce the smoke to a negligible amount and to widen the temperature range. It was, of course, clearly desirable to use the already available metal parts of the Aerojet unit insofar as possible. The specifications called for a unit which could be procured readily, which would give a thrust of a thousand pounds over a period of at least eight seconds (preferably twelve), and which would perform satisfactorily over a wide temperature range, especially at low temperatures. The exhaust gases from the unit should have a fairly low temperature, and preferably be without any hot solid particles. If possible, the gases should be free from smoke. Moreover, the propellant should be one easily procured through ordinary commercial channels.

These were rather drastic requirements. Back in 1942, no one knew how to make a propellant with the necessary low-temperature coefficient, smokelessness, and other needed characteristics. The fundamental research on propellants, which Section H had initiated early in 1942, now enabled the scientists to proceed successfully. With the knowledge gained from the fundamental studies on scores of experimental compositions, the scientists of Section H were able to formulate a new powder especially for the JATO job. They were actually able to write down a recipe for a new type of smokeless powder which would be slow-burning, probably very little affected by temperature, would give relatively cool exhaust gases, and would have the requisite ease of manufacture and rugged mechanical properties needed for the JATO unit.

Experience in the "restriction" of propellants also paid off. No propellant of normal form would give a burning time as long as eight seconds. Some form had to be developed so that the burning surface of the powder would remain constant, while the powder burned through a very long distance — and gave the long burning time needed. Web thicknesses of about seven inches were indicated. The NDRC scientists were able to design and develop a "cigarette-burning" cylindrical powder stick.

Of course, there was a considerable amount of work yet to be done. The brand new propellant composition had to be obtained and thoroughly tested to be sure it would perform as its designers specified. It was found satisfactory in all respects. The various types of restricting coatings which had been previously developed for other jobs, and as part of the fundamental research program, had to be tried out for this particular job. Some of them were not suitable; but one of the restricting methods which had been the subject of considerable study, proved to be adaptable to the JATO needs. A new method of loading had to be worked out which would permit the use of powder grains in a size that could be manufactured. Then such

devices as proper "traps" for the propellant charge had to be worked out. Moreover, arrangements had to be made for securing a sufficient quantity of the necessary component parts for testing. Here the available powder chamber and igniter system of the GALCIT unit proved a tremendous advantage. The availability of these units outweighed the disadvantage of having to fit the propellant charge and the general design to them. By April 1, 1945, these difficulties had been met, tests had been run, and about forty full-scale units had been fired over a wide temperature range.[3] Every unit had functioned properly as designed — the shear-disk safety valves had never been needed! Perhaps the only complaint that could be made against these units was that they were not completely smokeless. However, they were far better than the service Aerojet units; the smoke was a light tenuous one which blew away quickly, rather than the dense white clinging smoke of the service units. There were no solid particles in the blast. The light smoke from the newly developed unit did not coat objects in the path of the jet. On April 4, 1945, representatives of the Navy attended a demonstration of the new charge for the JATO unit. Five weeks later, the first dozen rounds of the completed JATO unit were shipped to the Navy. On June 1 and 2, flight tests were made. The new unit proved satisfactory, meeting all the initial specifications except, of course, that it was not completely smokeless. Less than six months had elapsed from the initial request to success.

This concludes the account of the high lights of the accomplishments of Section H. Any scientist reading between the lines will have perceived what may not be so obvious to many a layman, that behind these occasionally rapid successes lay a great deal of painstaking fundamental research, research whose results lay in the larder, ready to be called upon on a rainy day. A great deal of fundamental accomplishment, of greater future use than any of the weapons which met with success, is to be found in the deposited records of ABL. Even a listing of the subjects would be largely unintelligible and surely not very interesting to most readers of this volume. For those who have a further interest the record is on deposit in individual papers and in a summary report prepared by Dr. Gibson on the status of ABL projects at V–J Day.[4] In the long run this type of knowledge will be of greatest value, more than the bazooka, the 4.5-inch rocket, the American Donnerkeil, the jet-assisted take-off, the flame-thrower propellant, or the recoilless gun. For the engineer a decade hence, they and not these more colorful achievements will weave the laurel wreath for ABL.

[3]Service temperature range is given as —40 to 120° F.
[4]Pertinent excerpts will be found in Appendix 3 (p. 227).

# Work at the California Institute of Technology

## CHAPTER XV

### *THE EARLY WORK OF THE CALIFORNIA INSTITUTE GROUP*

THE CONTRACT for work at the California Institute of Technology (OEMsr-250, effective September 1, 1941) indicated the work to be undertaken as ". . . studies and experimental investigations in connection with the development, adapting, and testing of ordnance devices . . . in accordance with instructions of the contracting officer or his authorized representative," a rather more general description than that outlined by Section H at its August meeting. The contract also required the Institute group to provide drawings, specifications, instruction books, and reports covering the results of their work.[1]

Lauritsen returned to Pasadena as Director of Research for the project. NDRC appointed his colleague in the physics department, E. C. Watson, official Investigator (i.e., administrative head of the contract). Lauritsen and Watson drew heavily upon the Institute staff in setting up the supervisory nucleus of the rocket group. W. A. Fowler, Assistant Professor (now Professor) of Physics, was appointed Assistant Director of Research. Other staff members conscripted at this time were I. S. Bowen and W. R. Smythe, Professors of Physics, W. N. Lacey, Professor of Chemical Engineering, Bruce H. Sage, Associate Professor (later Professor) of Chemical Engineering, and D. S. Clark, Assistant Professor (later Associate Professor) of Mechanical Engineering and Metallurgy. Provision was made for reporting the results of the work by setting up an editorial section headed by Joseph A. Foladare, an Institute graduate who had transferred his interests from physics to English and was then Associate Professor of English at Santa Barbara State College. As rocket work expanded, additional supervisory

[1]This first contract was for $200,000. It was superseded and replaced by OSRD contract OEMsr-418, which became effective March 1, 1942. OEMsr-418, to which sixteen supplements were written, financed all the subsequent work of the Institute group. The funds provided by these contracts reached a total of $80,624,000.

personnel were recruited from the Institute staff, graduate students, and alumni, from the Mount Wilson Observatory of the Carnegie Institution, from other educational institutions, and from the business and professional community at large.

Headquarters of the group was the Kellogg Radiation Laboratory on the Institute campus in Pasadena. This provided office and drafting space, as well as a special shop for the section concentrating on fuze development. Later, the Kellogg office and drafting facilities were augmented by additional space in other Institute buildings and by quarters rented or bought in some twenty-odd different locations in Pasadena. For shopwork on the campus, they could call on the well-equipped Astrophysical Machine Shop of the 200-inch telescope project, as well as the smaller shops maintained by several departments of the Institute for the construction and maintenance of research equipment. Local commercial shops could also be used for additional work as needed.

Off-campus facilities were provided by the lease of a few acres in Eaton Canyon to the northeast of Pasadena. The Eaton Canyon tract provided suitable space for presses for the dry extrusion of propellant, for static test firing of propellant grains, and for a limited amount of free firing of experimental grains. As the work of the propellant section increased, the Eaton Canyon facility was enlarged by further leases until it included 184 acres. Through the co-operation of the Commanding Officer of Camp Haan, range space for more extensive test firing was made available on the Mojave Antiaircraft Artillery Range (later designated Camp Irwin), which had recently been established in the Mojave Desert some thirty miles north of Barstow, California, as an adjunct of Camp Haan.

The problems which the group started working on in September 1941 had already been formulated by Lauritsen's memorandum to the Director of OSRD and by the discussions and informal indications of Army and Navy interest in rocket weapons and devices which had taken place at the August meeting of Section H. The basic problem was to work out the apparatus and techniques for dry extrusion of double-base powder (ballistite) for rocket-propellant grains. The specific problems were the development of a target rocket, in which the Coast Artillery was interested for the purpose of training antiaircraft gunners, and the development of medium- and high-altitude antiaircraft rockets. Two more problems were added early in November, when the Chemical Warfare Service requested the development of a rocket-propelled chemical warfare bomb and an antitank grenade.

The first of the projects listed for the Cal. Tech. group, the dry extrusion of ballistite, was most urgent and fundamental because the development of antiaircraft rockets, as well as other high-performance rockets which could be foreseen as desirable, depended on thick-web propellant grains to provide the velocity and range required.

The English rockets[2] which had so favorably impressed C. C. Lauritsen in the summer of 1941 used thick-webbed, dry-extruded grains of cordite, a double-base powder with nitroglycerin and nitrocellulose as principal ingredients.

Thin sheet ballistite which was manufactured in the U.S. for trench mortar charges offered the same possibilities for dry extrusion as English cordite, and in its essential characteristics was quite suitable as a rocket propellant.[3]

It was desired to retain[4] the advantage of its higher heat of explosion and rate of reaction. The problem was to demonstrate that it could be dry-extruded as a practical process and without undue hazard.

The responsibility for designing a suitable press for quantity production of ballistite grains, supervising its construction, and getting it into operation was undertaken by Lacey and Sage. However, in order to demonstrate as quickly as possible the feasibility of the dry-extrusion process, Thomas Lauritsen[5] was assigned the job of getting an emergency press built and operating as quickly as possible. He had it built and turning out satisfactory 15/16-inch-diameter grains in about two and a half months.

Because of the urgency, no time was taken to develop a finished and final design. The press was built in one of the Institute shops out of materials and parts that were readily available — some of them, it is rumored, salvaged from junk yards. When it was finished, it was mounted horizontally on a trailer made from the back axle of a defunct Ford, and towed to Eaton Canyon for the first trial. In its essentials, the press consisted of a cylindrical chamber designed to be charged with a three-inch carpet roll of sheet powder, with a die at the exit end which would form the powder, as it was extruded, into a cylindrical stick 15/16 inch in diameter. For heating the cylinder during extrusion, a direct electric heating system was first tried, but was later replaced by hot-water coils. Means were also provided for exhausting the air from the cylinder before the extrusion process was started. The ram or piston which forced the powder through the die was driven by a 30-ton hydraulic jack.

In Eaton Canyon the press was set in the open, with the exit end pointed into a recess in the canyon side. Remote controls led over to a sandbag barricade, behind which the operators crouched, with a telescope poked

[2]See Report of C. C. Lauritsen in Chapter VI.

[3]Chapter II has a brief discussion of propellant burning characteristics. In Chapter V early consideration of the propellant problem is described.

[4]Ballistite, a hotter powder than cordite, has a higher percentage of nitroglycerin and the nitrocellulose has a higher nitrogen content.

[5]C. C. Lauritsen's son. At the outbreak of the war he was doing research work in nuclear physics under Niels Bohr in Copenhagen. When he returned to the United States, he, with Fowler, worked on the proximity-fuze project in Washington until the rocket group was set up at Cal. Tech.

between the sandbags so that they could read the pressure and temperature dials.

The first trial was made on November 15, 1941. The results were not completely satisfactory, but they were encouraging. Further trials were made on the 16th and 17th. On the latter date, a good homogeneous 6-inch stick was obtained from the first carpet-roll charge; on the second, too much pressure was applied, and the cylinder burst.

Repairs were completed by the 20th, and on that date, as the report reads, "A good straight stick about 30 inches long was obtained." Work stopped that day, not because of any failure in the press but because of a sandstorm that was howling through the canyon.

This press, which was dubbed the Little Giant, immediately went into production operation. By December 11, it had extruded all the available sheet powder, 180 pounds, into 15/16-inch-diameter sticks, most of which were used in static burning tests to secure fundamental data on propellant behavior. It continued in operation until more permanent press facilities were available.

The Little Giant was an emergency job. As such, it served its purpose well, but there was obvious need of additional extrusion facilities and especially of presses which would extrude thicker-web propellant grains. Such presses were being designed while the Little Giant was making its debut, and early in 1942 a 3-inch and a 5-inch press went into operation. The 5-inch-diameter press, which first demonstrated the feasibility of extruding grains as large as 2.5 inches in diameter, was used for the production of propellant grains for experimental rockets and later for Service rounds. By the end of the winter of 1942 an 8-inch press was operating which could extrude grains up to 3 inches in diameter. At the end of February 1942, the propellant section reported that they had extruded 1500 pounds of sheet ballistite, and were then running the presses at the rate of 200 pounds per day. Later, additional 8-inch presses and a 12-inch press were added. From the fall of 1943 until the early summer of 1945, one 12-inch and three 8-inch presses were kept in practically continuous operation.

In basic principle, all these presses were like the Little Giant. Each, as might be expected, embodied improvements in design over its predecessor: added safety features; complete remote control, with the dials recording temperature, pressure, ram position, and rate of extrusion in the control house; automatic means of cutting the extruding grain at any desired length; and so on. All the presses subsequent to the Little Giant were adequately housed, and all were designed for vertical operation.

The latter feature was dictated partly because of the terrain of the Eaton Canyon site, but more by the fact that with a vertical press, time can be saved in charging the extrusion chamber and powder can be used more economically. A horizontal press can be charged only with carpet rolls, a

necessity which involves considerable time in preparing the powder. For charging a vertical press, the sheet powder is cut into pieces about the size of calling cards, a process which can be carried on more rapidly than can forming carpet rolls. Furthermore, scrap material — the bits left over when the extruded grain is cut to exact length, and chopped-up grains which have been rejected for imperfections — can also be used for charging a vertical press. This saving of powder was particularly important during the first two years of the work by the California Institute group, for the availability of sheet ballistite for rockets continually lagged behind experimental and Service requirements.

While the battery of extrusion presses was being built up in Eaton Canyon, additional dry-extrusion facilities were being built up elsewhere — one at the Naval Powder Factory, Indian Head, Maryland, which was designed and built by the Institute group, and others at various plants of the Hercules Powder Company. But until well into 1944, dry-extrusion facilities were limited enough to require the Eaton Canyon plant to produce propellant grains not only for experiment and development, but for Service use as well. In fact, the bulk of the production was for the latter purpose.

This expansion of propellant production in Eaton Canyon meant also a parallel expansion of related activities. Facilities had to be supplied for preparing the powder for extrusion, processing and inspecting the finished grains, and static-firing samples from all production lots to make sure that performance measured up uniformly to acceptance requirements. Eaton Canyon activities also included production of igniters, and propellant loading and assembly of both experimental and Service rounds.

The first project which the group undertook was the development of a target rocket. No target rockets were ever fired at the enemy unless possibly by accident. Nevertheless, they made a significant contribution toward winning the war.

In the summer of 1941, when NDRC circularized the Armed Services as to their interest in a variety of proposed rocket projectiles and devices, Major General Joseph A. Green, Chief of the Coast Artillery, answered that his command would like to have a rocket target developed. The Coast Artillery Corps had the responsibility for training antiaircraft gunners. Something was needed which would give gunners adequate practice in firing at targets which approximated the speed and course of aircraft. One possibility was target drones — small planes directed by remote control. But target drones were not then in quantity production; they were expensive; and they were not fast enough. The conventional towed sleeve target was too slow and was towed on too steady a course to give the necessary training in leading a high-speed, maneuvering target. A rocket-propelled target, it was believed, would meet all the necessary requirements.

During the summer and early fall of 1941, some development work was done by California Institute staff members then at the Bureau of Standards in co-operation with the Coast Artillery, and a preliminary model was standardized. The California Institute group, however, felt that improvements could be made in design and performance, and when they started work in Pasadena, they were assigned the target rocket as the first object of extensive experimentation.

There were several advantages in beginning with the target rocket. It was a less complicated problem than the development of artillery rockets. The main requisites were velocity and visibility. The former was a matter of motor design; the latter could be taken care of by making the stabilizing fins large enough to serve as a target. Hence the fins themselves constituted the pay load, and the complication of the conventional head and fuzing were out of the picture. In general, the experience which the group would get in designing, building, and testing the motor and in firing the target rockets would give them useful data on motor behavior and rocket flight which could be applied to more difficult problems later.

The work was handicapped at first by propellant troubles. The first dry extrusion was not successfully accomplished until November 20, 1941. Even then, the supply of powder was small, and there were many demands on it for other work. Permanent presses were not operating until early in 1942, and there was not enough sheet ballistite for even small-scale experimental work involving dry-extruded propellant until February 7, 1942, when 2000 pounds of sheet powder were finally secured.

In the interim, the group got along with substitutes. They made motors using centrally perforated disks cut from thin sheet ballistite, coated with solvent, and pressed together to form a hollow cylinder. These never were satisfactory; the bonding between the disks was not complete; the powder grains burned irregularly and either blew up the motors or broke up and blew out the nozzle. Other propellant grains were made by wrapping solvent-coated sheet ballistite tightly around a mandrel, and then machining the grain to uniform diameter. But these, like the stacked-disk grains, burned irregularly and fissured. Still other propellant charges were made by stringing a number of thin-web, solvent-extruded powder grains on the wires of a cagelike arrangement (a so-called trap) to hold them properly spaced in the motor tube. These were better, but the solvent-extruded grains at that time were not completely reliable. They sometimes contained air bubbles and internal fissures which, in the course of burning, produced a sudden increase in burning area with a resultant blowup or breakup of the propellant. It was not until the Eaton Canyon presses were turning out a dry-extruded cylindrical grain 2½ inches in outside diameter, with a web thickness of ¾ inch, that the propellant problem was finally settled.

In spite of propellant difficulties, however, the target-rocket development proceeded rapidly enough so that on November 29, 1941, three rounds were launched at the Mojave Antiaircraft Artillery range for a firing detail of the 78th Coast Artillery to shoot at. This was a start on a very small scale, but about a week later, on the same range, target rockets were supplied for part of a National Guard antiaircraft regiment which fired 1000 rounds of ammunition from two 37-mm. guns on antiaircraft mounts. Officers and men considered the practice sufficiently valuable to schedule further practice for December 12 and 19, with the plan of firing some of the target rockets at high angles (about 60°) so that the latter part of the trajectory would simulate a dive-bombing plane, and some at low angles (about 30°) to simulate hedgehoppers.

Space limitation prevents following the rest of the story in detail. Target rockets were fired in constantly increasing numbers, and at various antiaircraft gunners' training camps in southern California and elsewhere. The verdict of officers and men was uniformly favorable. By the summer of 1943, for example, the target-rocket range at the U.S. Marine Corps' Camp Pendleton was scheduled for four weeks in advance.

This demonstrated usefulness of the target rocket justified continuing the development through 1942 and 1943, to give improved performance, better visibility, and the design of launchers suitable for general Service use. By December 1943 the Navy's Bureau of Ordnance standardized and undertook the production of two types of target rockets and several launchers developed by the California Institute group. In the meantime, while the Institute was carrying on the development and producing its own target rockets, some 21,000 officers and men had participated in this training all over the United States.

The target rockets as finally standardized consisted essentially of the motor plus three large fins, the latter demountable for shipping and stowing, and capable of being quickly attached to the motor. One model had a speed of about 425 miles per hour and a range of 1700 yards; the other, a speed of 300 miles per hour and a range of about 1130 yards. Both could be equipped with a nose whistle — an echo of the days when German Stuka dive bombers added sound effects to their other terrors; but these whistles, it is believed, never saw much Service use.

The launchers all had as their essential feature a 10-foot launching rail. In one variation this rail was mounted on a two-wheeled trailer for land use; in another, the rail was mounted on a collapsible metal frame which could be set up either on land or on shipboard.

When the group began work in September 1941, the development of medium- and high-altitude antiaircraft rockets was one of the principal projects assigned to them. At a meeting of Section H, Division A, NDRC,

late in August 1941, the Army representative had reported that next to a plane-to-plane rocket, the Ordnance Department considered the development of antiaircraft rockets highest on the list in urgency.

The Navy was reported to believe that antiaircraft rockets were "of importance," but of more interest to the Army than the Navy. The rocket projects to which the Navy gave the highest priority were already underway at Indian Head or elsewhere, and work on the plane-to-plane rocket was also being carried on at Indian Head, where the NDRC group (Section H) was even then finding that the current program put a severe load on facilities and personnel. Therefore, the development of the antiaircraft rockets fell to the newly organized group to begin work at the California Institute the following month.

There was another reason why antiaircraft-rocket development was logically assigned to this new group. A thick-web propellant grain was essential to the development of a high-performance rocket, especially such a one as the high-altitude antiaircraft rocket. English experience with cordite had shown that such a grain could be produced by the dry-extrusion process; and as has already been pointed out, one of the main reasons for setting up the group at the Institute had been to develop presses and techniques for the dry extrusion of ballistite, which was the closest equivalent to cordite that was being produced in the United States.

Like the English, we were necessarily thinking at the time primarily in terms of defense. After Pearl Harbor, the coastal blackouts and the elaborate organization and training of civilians for airplane spotting, fire fighting, and first aid were evidence enough that one of our principal fears was attack from the air. The British were in the same situation, only their crisis had come earlier, and the air attacks had materialized.

With the outbreak of the war they found themselves desperately lacking in antiaircraft artillery. There was only a pitifully inadequate number of antiaircraft guns to defend vital harbors, factories, military installations, and shipping. The only available substitute was rockets. Fortunately, their rocket development had begun earlier than ours, and they already had enough knowledge and experience so that they could put into production rockets designed to meet specific anticipated needs. For the first year or so developments were directed toward counterweapons for dive-bombing attacks; and the role played by the Stukas in the debacle in France strengthened this preoccupation.

At the request of the Admiralty, a light antiaircraft rocket with a head 2¼ inches in diameter had been developed for the defense of unarmed merchant vessels and aircraft carriers. This was in production in 1940, and extensive installations were made on merchant shipping. Concurrently with this, another development had been made which was also designed to

protect ships. This was the naval wire-barrage rocket, which went into use early in 1940. Its name is sufficiently descriptive of its purpose: it was designed to carry wire aloft, with fatal consequences to any enemy airplane that attempted to fly through it. H.M.S. *King George V,* when it brought Ambassador Lord Halifax to the United States, gave a limited number of Americans their first view of the wire-barrage rocket and its launcher: American Navy officers who boarded the British battleship in New York harbor saw an installation mounted on the stern deck.

Unfortunately, the wire-barrage rocket, when fired from shipboard, proved to be a hazard to its own side as well as to the enemy, since the pay load of wire sometimes fouled the ship from which it was fired. From land-mounted launchers it acquitted itself better. Installations were set up on the Dover cliffs in July 1940, after Dunkirk, and the wire-barrage rockets were fired with some success against German planes. It was replaced in 1941 by another weapon called the "fast aerial mine."

The high-altitude antiaircraft rocket, commonly known as the British UP-3 (unrotated projectile, with 3-inch-diameter motor and head), was begun earlier than the types of rocket just discussed, but did not go into Service use until 1941. Extensive installations of Z-guns, as the launchers were called, were not ready for use until after the Germans had given up heavy night bombing in the spring of 1941.

It was the British UP-3 which had particularly impressed C. C. Lauritsen, when he was in England in the spring of 1941. Since our own highest-priority requirements in the fall of 1941 and through the first half of 1942 were primarily for defense, it was natural that antiaircraft weapons — rockets among them — should stand high on the list, and that the development of antiaircraft rockets as undertaken by the California Institute group should follow the pattern of British experience.

Before an effective antiaircraft rocket could be developed, however — particularly a high-altitude rocket — a thick-web propellant grain was necessary. So, while the propellant section was designing and installing presses and accumulating experience in the technique of dry extrusion, the projectile section of the Institute group was able to make only tentative approaches to the antiaircraft rocket problem with the propellant that was then available. While they designed some models and fired them on the range as early as December 1941, this experience was principally useful as giving additional information about some of the critical relationships in motor design. No work on a suitable Service head and fuze was ever undertaken. During the first part of 1942, this work was continued, but at fairly low priority, partly because of the propellant situation, partly because another project had taken top priority.

By the time a suitable propellant grain was available for high-perform-

ance antiaircraft rockets, the fear of the sort of attack which had motivated the antiaircraft-rocket program had passed, and so the development was never started in earnest.

Early in November 1941 Watson, while in Washington for a meeting of Section H, discussed with the Chief of the Technical Service of the Chemical Warfare Service the possible requirements for rocket propulsion of chemical weapons. Two possible requirements were indicated: a chemical warfare rocket[6] and a rocket-propelled antitank grenade.

At that time, the Chemical Warfare Service had already had reports on a 5-inch chemical warfare rocket which the British had developed as a defensive weapon for the anticipated invasion of England. There would be great advantage, the Service believed, in the development of such a weapon as a possible substitute for the standard Livens projector. Because of the lower setback and acceleration forces, the rocket head could be made with a lighter, thinner wall and hence would carry a greater amount of chemical filler. The absence of recoil would permit a relatively lightweight launcher. This meant that the weapon could be transported by and even fired from a standard truck, thus eliminating the weight of the Livens mortar and the time required to emplace it. Finally, the light weight and mobility of the equipment would permit the rapid laying of an effective concentration of nonpersistent gas in tactical situations where this would be impossible with the Livens.

The Chemical Warfare Service wanted a projectile having the general characteristics of the Livens, but preferred a range of 3000 yards (the Livens range was 2000 yards), and would be agreeable to reducing the chemical filler in the head from 30 to 21 pounds in order to secure this range. The accuracy should be not less than that of the Livens, the probable error of which was 50 yards in range and 25 yards in lateral deflection.

Tentative specifications for such a rocket were agreed on at a conference in Washington. Shopwork was under way in Pasadena the following week, and the first models were test-fired at the desert range.

Like all the other rocket developments in the fall of 1941 and the early winter of 1942, the Chemical Warfare rocket was slowed up by the shortage of sheet ballistite for dry extrusion. But by mid-April 1942 the development of the projectile was pretty well completed and work was undertaken on a launcher for Service use. In the middle of July, a test firing at Edgewood Arsenal was sufficiently promising to warrant a request for 600 rounds for Service test. A request was also made for a new launcher built to meet specifications set up by the Field Division of the Chemical Warfare Service.

These requirements were based on the following assumptions. The launcher would be stored in rear areas for use in temporarily static situa-

[6]Originally called the Chemical Warfare bomb.

tions. Truck transportation would be required to the vicinity of the place of firing. However, tying up transportation by installation of the launcher on a special truck or trailer would not be warranted. Hence, it was recommended that the launcher development be based on a multiple-unit model, capable of firing a salvo of at least 12 rounds, and capable of being fired either from the standard vehicle available to the unit (a 6 x 6 general-purpose truck), or from the ground.

Two launchers were developed along the lines laid down by the Chemical Warfare Service. Both were cratelike arrangements containing multiple launching rails and capable of being quickly installed in or demounted from the rear part of the cargo space of the truck. One launcher had a capacity of 16 rounds; the other, 24 rounds. After preliminary firings, Service tests were held at the Dugway Proving Ground in Utah, January 25 to February 3, 1943.

The official report on these tests stated that the 7-inch chemical rocket was a suitable ammunition for area coverage, and recommended that it be standardized at once to replace the Livens projector. The 24-round launcher was preferred, but the report recommended that it should be strengthened for greater rigidity. It was further recommended that work be done to reduce the dispersion in firing, and that firing tables be prepared.

When the projectile was standardized, the Chemical Warfare Service would proceed with the manufacture of the metal parts through its Industrial Division, and would arrange for a supply of powder for propellant. In June 1943, Army, Navy, and NDRC representatives meeting in Washington decided to modify the design somewhat to bring the Chemical Warfare rocket more nearly into line with other rockets which the California Institute group had in the meantime developed. The principal change was to increase the diameter of head from 7 to 7.2 inches. July and August saw extensive tests of this new model. To provide ammunition for Service trials the Bureau of Ordnance of the Navy requested the California Institute to produce 8000 rounds for Marine Corps use. This production was largely completed by the fall of 1943. In the meantime, the Ordnance Department of the Army had started production of the round and had requested the Institute to load 4000 motors — the propellant grains to be supplied by the Radford Ordnance Works, which, by this time, had facilities in operation for the solventless extrusion of sheet ballistite.

The Chemical Warfare rocket, as far as is known, never saw Service use. Like certain other weapons, it was developed as a hedge against possible use of gas attacks by the enemy. Since this contingency never occurred, the Chemical Warfare rocket served its purpose only as a kind of insurance.

The second weapon in which the Chemical Warfare Service was interested was the antitank grenade (later designated the incendiary rocket

grenade or the Chemical Warfare grenade). An antitank shoulder weapon was already under development by Section H at Indian Head. This finally evolved into the bazooka, which carried a high-explosive head and depended on the shaped-charge principle for its lethal effect. In addition to this, however, the Chemical Warfare Service wanted a rocket weapon designed to carry one pint of incendiary material in a frangible container, with fuze and burster tube.

Work was started at the California Institute and the Bureau of Standards. After preliminary work on motor design, the first Institute models were ready for test firing at the desert range. Then the development went through a long series of modifications and tests. The principal problem was one of motor design. The first propellant grain used was a ballistite cylinder with outside diameter of 1 inch, inside diameter ¼ inch. This was satisfactory as far as range was concerned, but the relatively large web thickness meant a correspondingly long burning time, and this, in turn, produced too large a dispersion. To remedy this, a shorter burning charge was devised. This consisted of two concentric ballistite cylinders, the outer one 1-inch outside diameter by ¾-inch inside diameter; the inner cylinder ½-inch outside diameter by ¼-inch inside diameter. This cut down the dispersion, but the motor tube had to be lengthened because the necessity for leaving adequate space for gas flow between the adjacent ballistite surfaces prevented as high a density of powder loading as in the single-grain motor.

The development of this weapon continued all through 1942, though other and more urgent projects prevented it from going ahead on the highest priority. In 1943 the design was finally stabilized, using a single cylindrical powder grain of 1-inch outside diameter and ½-inch inside diameter, which, in the meantime, had been developed for other purposes.

There was considerable Service interest in this weapon for a while, but tests finally proved that the pint of chemical material which the rocket carried was too little to have the desired effect, and the Chemical Warfare grenade, also, was never adopted for Service use. Nonetheless a good deal of experience had been gained which would come in handy later.

# CHAPTER XVI

# *THE MOUSETRAP AND MINNIE MOUSE*

IN LATE JUNE 1942, a telegram addressed to C. C. Lauritsen at Pasadena was filed at Key West, Florida. It read: "Mousetraps arriving on schedule; Minnie Mouse urgently needed."

Cryptic as this message may have seemed to the telegraph operator, to the initiated it meant that the California group was well along in another development project — had, in fact, reached the point where a new type of rocket, which had been demonstrated to the Navy only a few weeks before, was being rushed into Service installations, training, and operational use. This was the rocket-propelled antisubmarine bomb. It was the first Institute rocket to be fired against the enemy.

The rocket-propelled antisubmarine bomb was developed in response to one of the most urgent needs of the early years of World War II. Even before Pearl Harbor we were in fact engaged in antisubmarine warfare through the passage of the Lend-Lease Act, the decision to help the British in convoying supplies in North American waters, the submarine attack on the United States destroyer *Greer* on September 4, 1941, and the subsequent order to the naval forces to shoot on sight any vessel attempting to interfere with American shipping or any shipping under American escort.

Whatever the technicalities of our status, as the United States moved from insulation and isolation in 1939 to effective belligerency in the fall of 1941, the Navy had early taken a realistic view of the situation and was getting ready for foreseeable contingencies. Antisubmarine warfare was one of the most obvious. In the fall of 1940, the Navy requested the National Academy of Sciences to make an impartial appraisal of the Navy's equipment and techniques for antisubmarine warfare. The Academy appointed a civilian committee, which turned in its report in February 1941. Among the committee's recommendations were three which had an important bearing on the Institute's later development of the rocket-propelled antisubmarine bomb. These recommendations were: (1) the establishment of two new laboratories, one at New London, Connecticut, the other at San Diego, California, which were to have as one of their principal functions the development of more reliable apparatus for underwater-sound detection and sound ranging; (2) the use of large numbers of small craft in antisubmarine warfare; and (3) the development of the so-called forward-thrown attack.

The dropping of depth charges was the standard antisubmarine attack in World War I. When the operator of the underwater-sound gear reported a submarine contact, the attacking ship maneuvered to drop a pattern of depth charges which were fuzed to explode at various distances below the surface. The depth charges were not intended to destroy the enemy submarine by explosion upon physical contact. Rather, since they were distributed in an area pattern and set to explode at different depths, they would create an area of violent shocks strong enough to crush or rupture the hull of any submarine caught within this lethal zone.

In World War I, the depth-charge attack was successful in bringing convoys of troops and supplies safely through submarine-infested waters, and was credited with a large number of kills. But when the war was over and the score could be accurately checked, the actual number of kills shrank to about one third of the previous estimate.

One of the main reasons for this discrepancy was probably the fact that the sound gear becomes increasingly inaccurate as the attacking vessel nears a position directly above the submarine. Hence, there is a time interval between comparatively reliable signals from the sound gear and the moment when the depth-charge attack can be started. This interval is not long, but it may well be long enough for the submarine to take successful evasive action or for the attacking vessel to mistake the position of the submarine. After depth-charging has begun, of course, the underwater explosions make the sound gear useless, and the rest of the attack must depend upon luck and intuition.

In the opinion of the Academy committee, these problems could best be solved by an attack in which some sort of antisubmarine projectile could be fired forward from the attacking ship from a range of a few hundred yards, while sound contact was still reasonably reliable. Hence the designation forward-thrown attack.

When the exchange of scientific information with England began in 1940, NDRC and the Navy learned that the British had also analyzed the weaknesses in the depth-charge attack, had concluded that a forward-thrown attack was the best solution of the problem, and had already developed several projectiles for this type of attack. Two members of Section C-4 of Division C, NDRC, which had been organized to deal with problems of subsurface warfare, went to England in the spring of 1941 to get a firsthand view of British antisubmarine weapons and methods. They brought back with them drawings of one type of projector developed for the forward-thrown attack, the so-called Hedgehog.

The Hedgehog got its name from its fancied resemblance to the eponymous animal with its spines bristled up. It consisted in essentials of a deck frame supporting a number of so-called spigots — that is, steel rods which were inclined in a graduated spread forward and outward.

The projectile had a head containing high explosive and an appropriate fuze; into the base of the head was screwed a piece of tubular steel, with stabilizing fins at the rear end. In loading the Hedgehog, the tube, which constituted the body of the projectile, was slid down over the spigot. In firing, the closing of an electrical contact ignited a charge of smokeless powder loaded into the forward end of the tube, and the projectile was shot out ahead of the ship. The whole arrangement constituted a very simple sort of gun with the spigot serving to provide a base against which the powder charge could react and to guide the projectile at the start of its flight. Since it was essentially a gun, it involved considerable recoil — a factor which set a lower limit to the size of ships capable of using Hedgehog equipment.

Unlike depth-charging, Hedgehog attacks depended for their effectiveness upon actual hits. With the spigots set at varying angles, the projectiles would fall in a dispersed pattern. If the sound ranging was accurately interpreted, this pattern was such that there was a reasonable certainty of one of the projectiles scoring a hit as it sank through the water. The head was fuzed to explode on contact, even at oblique angles; and the 30 pounds of high explosive was sufficient to blast a sizable hole in the pressure hull of a submarine.

The Navy, as well as NDRC, was cognizant of the Hedgehog development and during the winter of 1941–42 was formulating a program for adaptation and procurement of Hedgehogs and projectiles for equipping of American vessels. So far, however, no provision had been made for equipping for a forward-thrown attack the "large numbers of small craft" which the Academy committee had recommended as desirable for antisubmarine warfare.

Still another of the recommendations of the Academy committee called for the establishment of two special laboratories. By the time the California Institute group started work in September 1941, the laboratory at San Diego was going into operation. The general administration and military control of the laboratory were the responsibility of the Navy; scientific and technical direction were in the hands of Section C-4, Division C, NDRC. Two members of the California Institute staff, Carl D. Anderson and William V. Houston,[1] were co-operating in the work of the laboratory. Still another member of the Institute staff, Max Mason,[2] was establishing, during the fall of 1941, facilities at Morris Dam, some seventeen miles from Pasadena, for the study of underwater ballistics of projectiles — bombs, depth charges, etc. This work was supported by OSRD Contract OEMsr-

[1]Each was Professor of Physics; Houston, in the early spring of 1946, assumed the presidency of the Rice Institute.

[2]Member of the Executive Council and Chairman of the Observatory Council; since the fall of 1945 a Trustee of the Institute.

329, and was under the technical and scientific cognizance of Section C-4, Division C, NDRC. (After Contract OEMsr-418 with the Institute became effective, the Morris Dam work was continued under it.)

The net result of all these activities, plus the fact that C. C. Lauritsen had also followed the English developments of the forward-thrown attack, was a group of workers interested in and aware of one another's problems, and close enough together geographically to exchange ideas and suggestions.

Consequently, as might be expected, there was considerable discussion of the possibilities of using rocket propulsion for forward-thrown antisubmarine projectiles, particularly as a means of increasing the effectiveness of small antisubmarine craft.

Finally, with informal authorization from NDRC, the Institute group, in the fall of 1941, began work on a rocket-propelled antisubmarine bomb. Like all the other rocket projects they undertook that fall, the antisubmarine bomb development was slowed by scarcity of suitable propellant. But various tests were made with substitutes — in solvent-extruded ballistite tubes, and propellant grains made from pressing together disks of sheet ballistite. Data were gathered on the amount of propellant necessary to propel heads of different weights; and some experiments were made with a motor which separated from the head when the burning of the propellant was completed.

The situation was considerably eased early in 1942, when a 5-inch extrusion press, capable of extruding propellant grains as large as 2.5 inches in outside diameter, went into operation. And early in February, 2000 pounds of sheet ballistite were secured — enough to keep the whole experimental program going for the time being. Test models were fired in Eaton Canyon and on the desert range. By March 3, 1942, the group could conclude that the rocket aspects of forward-throwing were pretty well worked out and the techniques practicable.

The Navy was being kept in touch with developments and on March 18 requested that the work be expedited and a report of the first sea test be submitted "at the first moment."

This first test was made March 30, 1942, off San Diego. Through the co-operation of Division C, use of the yacht *Jasper,* which was attached to the Sound Laboratory at San Diego, was secured as the firing ship. The target was a spar towed by an auxiliary boat on a 100-foot line, which, in turn, towed a marker on a 216-yard line. The latter was for the purpose of determining range for the rocketeers on the *Jasper*. The projectiles were approximate copies of the British Hedgehog projectiles as to form, tail, and position of center of gravity, but in weight they were increased to 86 pounds instead of the Hedgehog's 58 pounds. The heads, which were loaded up to weight with inert filler, were designed to hold 40 pounds of TNT for Service rounds.

The launcher was a simple arrangement of 6 standard 5-inch steel channel sections, each of which held one round. These rails were fanned out slightly from parallel, so that when the launcher was elevated at an angle of 45°, the 6 bombs would fall at a spacing of 17 feet apart, along a line 220 yards ahead of the ship and at right angles to its course. Since the launcher base was fixed in position, aiming had to be done by aiming the ship.

Shortly thereafter, somebody nicknamed the launcher the Mousetrap. With the rails inclined to 45° for firing, the launcher has in profile something of a resemblance to the old familiar nemesis of mice. The name stuck. It could be used without violating security, and it was certainly a great deal simpler than "projector for antisubmarine bombs." For some time, after the Mousetrap installations went into Service use, they were so designated, and the rocket-propelled antisubmarine bombs were simply labeled "Mousetrap ammunition."

This launcher was mounted on the port side of the *Jasper's* foredeck. For protection against rocket blast, the deck was sheathed with quarter-inch steel plate for about four feet behind the launcher; and behind the sheathing, water was flowed over the wooden deck during firing. The vertical wall of the charthouse was protected with light plywood.

A total of 20 rounds was fired; 10 single shots, a salvo of 4 fired at 1/10-second intervals; and a salvo of 6, with the same timing.

The object of this test was to see whether loading and firing could be carried out satisfactorily at sea, whether the projectiles had the desired characteristics of range and shot pattern, and whether the blast effect on the ship would constitute a serious problem.

All the projectiles fired satisfactorily. They fell a little short of the desired range, but this was believed to be due to stretching of the rope that towed the target and the range marker. Provisions against blast proved to be adequate, but slight improvements appeared to be desirable. Like many other early rocket tests, this one was on the whole successful, and at the same time it indicated further work that needed to be done. A Division C representative who was present reported that he considered the demonstration full of promise, and the difficulties and hazards of handling rocket-propelled antisubmarine projectiles not serious. He also reported that if the Navy was interested in going ahead with this weapon the California Institute group could begin supplying launchers and rounds immediately.

The Navy was definitely interested. The next thing to do was to try out the Mousetrap against the kind of target it would have in service—a submarine. Such a demonstration was scheduled off Key West on April 17, with the firing to be done from a Navy ship, *PC-449*, with the submarine *R-2* as the target. On the preceding day, to familiarize the crew of the PC with the general procedure, a few rounds of ammunition—by this

time the Institute had produced an improved model — were fired at a towed target. On the 17th, in addition to NDRC personnel from Division C and the Institute, Navy representatives were on hand from the staff of the Commander in Chief, United States Fleet, the Bureau of Ships, the Bureau of Ordnance, and the office of the Co-ordinator of Research and Development. Twenty-two rounds were fired from the Mousetrap at the submerged *R-2*, and three hits were scored. Reporting the test to the Commander in Chief, United States Fleet, the Commander, Service Squadron Nine, concluded:

> In view of the excellent impression made upon the officers witnessing this test, the fact that the "rocket" principle eliminated the recoil and much of the strain on the vessel . . . the fact that this type of projector [i.e., launcher] is so simple, light, inexpensive, and easy to assemble quickly out of stock materials, and assuming that the projectiles can be readily obtained, it is strongly recommended that all ships of PC Division Thirty-One and the Coast Guard cutters now assigned to this command be equipped with this armament as soon as possible.

Four days later, on April 21, the Vice-Chief of Naval Operations wrote to the Chief of the Bureau of Ordnance, stating that the Mousetrap appeared to be "a very effective installation," especially useful for ships too light to take the Hedgehog, and requested the Bureau of Ordnance to furnish this equipment for all 110-foot subchasers and all Coast Guard vessels of 83 feet and up which could not take the Hedgehog. The Bureau of Ordnance was further requested to arrange with NDRC for experimental equipment at the rate of one equipment per day until Bureau of Ordnance production could be established. The Bureau would be expected to make "such improvements as experience dictates," but to introduce these without delaying ship installations.

If the Mousetrap program had been proceeding at a high rate of speed before, this official action doubled the tempo. The California Institute group got production under way very shortly; by July 5 they had delivered about 60 Mousetraps and 2000 projectiles to the Navy. At the same time, they were continuing work on improvements in both launchers and rounds, and they developed a subcaliber practice and training round (once the Mousetrap had been so named, it was practically inevitable that the subcaliber round would be christened Minnie Mouse). They also supplied one member of their group, W. R. Smythe, who stayed at Key West through the whole summer to help with installation, training, and the evaluation of tactical procedures at the Navy Sound School.

The production problem could be taken care of, because in the original development of the Mousetrap, the Institute group had initiated procurement from the Lane-Wells Company of Los Angeles. To meet the Navy schedule, this contractor, who was already familiar with processing various components of the equipment, could step up production.

It is true that quantity production was not one of the purposes for which NDRC was set up; the normal procedure would have been to have the using Service undertake production when NDRC had finished the development. But in this case, the development was to continue even while Service installations were being made and Service use was beginning. NDRC was partly justified, then, in authorizing this production because it continued a development program. But the main justification lay in the submarine situation. The threat of enemy submarine attack was no longer something somewhere remote, off in the Atlantic. It had come to the East Coast of the United States, so close that coastwise shipping was not only threatened but under attack. In fact, on the day of the tests, while some Navy craft were trying out the Mousetrap, other Navy craft in the same area were rescuing survivors of a torpedoed merchant ship. There was desperate and urgent need of any weapon that promised to save American lives and American shipping. This was the main reason both NDRC and the California Institute group were willing to undertake the Navy's crash-production request.

In continuing the purely developmental part of the program, two important improvements were made in the propellant grain. For the Mousetrap motor, the propellant section of the Institute group had developed a tubular, dry-extruded grain of approximately 1¾-inch outside diameter and ½-inch inside diameter. In static firing tests, high and abrupt pressure peaks, in some cases twice as high as the average pressure values, were conspicuous in most of the firings. For the sake of safety, these high-pressure peaks must be eliminated. It was believed that they were caused by inequalities of pressure between the outer surface of the grain and the motor wall, and the central space of the grain. In order to equalize these pressures, John McMorris of the Projectile Design Section hit on the device of drilling radial holes through the walls of the tubular grain. This proved to be a satisfactory solution. The size, number, and arrangement of the radial holes were worked out by trial and error; 10 quarter-inch holes spaced evenly along the grain were found to be the most effective. This solution was found just prior to the first sea test at Key West.

The second improvement in grain design had to do with the problem of providing spacers to hold the grain evenly away from the motor wall, in order to supply sufficient and uniform clearance for the gases to flow freely back to the nozzle. In the early Mousetrap motors, three plastic strips were bonded longitudinally to the grain for this purpose. Then somebody had the happy idea of making the spacing strips an integral part of the grain itself—that is, designing an extrusion die with three notches around its outer diameter, so that the extruded grain would have three longitudinal ballistite ridges projecting the desired distance from its outer surface. This change was successful. It had the great advantage of eliminating the extra

processes involved in the old system of bonding the plastic strips to the grain, and hence speeded up production. The grains with extruded ribs were standardized for Mousetrap motors. This tubular 3-ridge grain with radial holes was not only standard for Mousetrap projectiles; it was used, with minor modifications, for other rockets which California Institute group developed later.

Launcher development resulted eventually in the standardization of two 4-rail launchers instead of the one 6-rail model used in the first sea test off San Diego in March. The 4-rail launcher consisted in a simple deck frame to which the launching rails were pivoted at the rear. For stowage, they were lowered flat and lashed to the deck frame. For firing, they were elevated to an angle of 45° with the horizontal, and aiming was accomplished by aiming the boat. Two such launchers, mounted one to port and one to starboard on the foredeck, constituted the standard installation. This gave each craft so equipped an 8-round salvo without reloading the launchers.

Tactical analyses by L. B. Slichter[3] demonstrated that a double-line pattern of 16 rounds could increase greatly the chances of a kill. To gain the advantage, the Key West group developed 8-rail double-decker launchers; these were ready for use by October 1942. As in the earlier installations, a pair of these launchers was mounted side by side on the foredeck of each ship.

Minnie Mouse, the subcaliber practice round, was an obvious necessity. As an extensive Service program developed for the Mousetrap and created a corresponding requirement for projectiles, the specter of a shortage of sheet ballistite was again moving on to the scene. The subcaliber round, with a much smaller propellant grain, would be perfectly adequate for training and practice, and would economize on the strained propellant supply. Mice have a notoriously short period of gestation; Minnie Mouse was no exception. A dry-extruded tubular grain was quickly developed. It was three-ridged like the Mousetrap grain, but only an inch in outside, a half-inch in inside diameter. The whole projectile was designed to have the same velocity, range, and underwater characteristics as the full-size Mousetrap ammunition. The head was steel of the required weight. A cavity in the forward end of the head held a shotgun shell and firing pin, so that in practice the occupants of the "tame" target submarine could tell by the explosion of the shell that a hit had been scored on them. Later, the shell was eliminated as the thud of the subcaliber round on the submarine hull was sufficient indication of a hit. The first model of Minnie Mouse was fired on the desert range; the test and training group at Key West were getting supplies of Minnie Mouse a month afterward. For

[3]Professor of Geophysics on leave from M.I.T. He is now Professor of Geophysics at the U. of Wisconsin.

firing, so-called adapter rails were quickly fitted to the standard rails of the Mousetrap launcher. They could be as quickly removed, leaving the launcher rails ready for full-size ammunition.

The Navy, meanwhile, had been carrying on its own program of domesticating the Hedgehog for American use. For a while, as the Mousetrap development came to the fore, there was, in prospect, the practical problem for the Bureau of Ordnance of producing and supplying at least two different varieties of ammunition designed for the same purpose. This difficulty was resolved on July 3, 1942, when it was decided that the same body design (i.e., head or pay load) and fuze were to be used for both Hedgehog and Mousetrap ammunition.

This decision needs no comment. Its advantages are obvious. The projectile which resulted from it was a light-case bomb with a total weight of 60 to 65 pounds, including 30 to 35 pounds of high explosives, TNT or Torpex. The range, with the launcher elevated to 45°, was about 300 yards.

At the California Institute end, work went steadily on, Institute production filling in until Bureau of Ordnance prime contractors could get under way. Fuze development, on which a great deal of effort was expended, will be described later. Improvements in design, resulting from studies carried on under Mason's direction at the Morris Dam laboratory, improved the underwater characteristics, especially the sinking rate, of the projectile. By early October 1942, the Key West training center could report a total of 100 Mousetrap installations made and a well-co-ordinated training program in progress, with sea demonstrations of Mousetrap attacks on tame target submarines.

There is no need of following the rest of the Mousetrap history in detail. The Mousetrap was in extensive use along the Atlantic coast and in the Caribbean by the fall of 1942, in the Pacific about six months later. The largest type of ship to be equipped with the Mousetrap was the destroyer escort (DE); and ships from that size down, even including harbor-patrol vessels, had Mousetrap installations.

The Mousetrap did not replace the depth charge. Rather, there was a more profitable division of labor between the two, with a great saving in depth charges. The standard procedure was to use the Mousetrap attack whenever a submarine contact was suspected. Mousetrap ammunition was fired until there was underwater explosion, oil slick, or other evidence of a hit. Then the depth-charge attack took over to finish off the submarine. For this reason, it is difficult to assign many kills to Mousetrap attack alone. But it can be credited with many assists, and when the war ended, it was still rendering effective service.

# CHAPTER XVII

## *FUZE PROBLEMS*

SO FAR, fuzes have been mentioned only incidentally. The California Institute group had first become involved in fuzing problems as they worked on the Chemical Warfare rocket. In this case, the problem was solved fairly easily by modifying a type of fuze designed for trench mortar projectiles. Fuzing the Mousetrap projectile, however, gave them their real initiation into fuze work. This, like many other initiations, meant wisdom gained through tribulation.

Since practically all the subsequent rocket developments of the Institute group involved fuze problems, it might be well to pause here to outline briefly the purpose and requirements of fuzes, and then to summarize the Mousetrap experience.

Fuzes are like the old-time Army mule, ornery but indispensable. The function of a fuze is to detonate the explosive in a projectile at the place and time where it will do the most damage. This detonation is usually initiated by driving a firing pin, at the critical moment, into a small quantity of an impact-sensitive explosive (called a detonator). Usually this sets off a small booster charge, which in turn detonates TNT or some other high-explosive bursting charge. If the head is loaded with smoke or chemicals, the detonation of the burster charge will shatter the case and spread the loaded material.

But projectiles and their fuzes must be handled for shipping and stowage; they must be assembled and loaded into guns or launchers; they may be subjected to severe jarring or to accidental drops. Consequently, fuze design must incorporate mechanical safeguards, to prevent the firing pin from striking the detonator all through handling and firing. But the design must also provide for the automatic removal of these safeguards after the projectile has been fired, so that the fuze will be free to function when the projectile reaches its destination. This latter process is called "arming" the fuze.

In fuzes for conventional artillery shells, arming is usually effected by a combination of setback and rotation. "Setback" is the name given to the forces arising from the acceleration which the shell undergoes when the gun is fired. Since the shell has reached its maximum velocity by the time it leaves the muzzle of the gun, these forces are sudden and violent. Hence, in artillery shells, this setback force can be utilized to remove the first set

of safeguarding devices. Usually a final step is accomplished by the centrifugal force of the shell as it rotates about its longitudinal axis. After the shell has rotated a certain number of revolutions and has traveled some distance in free flight from the gun barrel, the last impediment between detonator pin and detonator is removed and the fuze mechanism is free to function.

Conventional artillery fuzes cannot, however, be used for most rocket projectiles. The principal reason is the comparatively small setback force of rockets, particularly those using a thick-web propellant grain. Because of the comparatively long burning time of the propellant, the rocket reaches its maximum velocity much more slowly, and, in addition, that velocity is usually less than the velocity of an artillery shell. Consequently, fuzes for rocket projectiles must be designed along different lines embodying different principles. (Some projectiles with lower velocities, like mortar shells and aerial bombs, had fuzes which provided useful starting points for rocket fuze design.)

For the Mousetrap projectile, there seemed to be a good prospect in the early stages that there would be no fuzing problem at all. The British Hedgehog was fuzed, when information about it first came to this country, with the British No. 420 fuze. The original plan was to adapt this fuze for the Mousetrap round. But word came from England that a much better fuze, the British Type H, was imminent. Drawings of the Type H were flown from England. The California Institute group ordered the fabrication of Mousetrap rounds with a fuze cavity sized to take the Type H, and rushed through the construction of half a dozen Type H fuzes.

Unfortunately, they proved to be completely impractical for Service use because of overcomplicated design. The Bureau of Ordnance concurred in this view, and designed a fuze which was a modification of the earlier British No. 420. This the Bureau designated the Mark 31. In the Mark 31, the first stage of arming was accomplished by setback, the second by the rotation of a propeller in the nose during the first fifteen feet of underwater travel. This fuze was satisfactory for the American version of the Hedgehog, but for the Mousetraps the setback forces were too small to accomplish the first stage of arming, and the so-called setback collar had to be manually retracted.

Since this was obviously unsatisfactory, the Institute group undertook the design of a new fuze. This, like the ill-fated British Type H, was designed to arm by a buildup of hydrostatic pressure. It was designated the HIR (the first initial standing for "Hydrostatic," the method of arming; the second for "Impact," the method of detonating; and the third for "Rocket," the type of projectile).

At this stage in the proceedings a conference on antisubmarine ordnance for surface ships was held in Washington on July 3, 1942, in the office of

the Navy's Co-ordinator of Research and Development, Rear Admiral J. A. Furer. (This was the conference which decided to use the same head for Hedgehog and Mousetrap projectiles.) Participating in it were representatives from the staff of the Commander in Chief, United States Fleet; the Office of the Co-ordinator of Research and Development; the Bureau of Ordnance; the Antisubmarine Warfare Unit of the Navy; the Joint New Weapons Committee; and Divisions A and C, NDRC. Among other things, they decided that the Mark 31 fuze (the Navy's version of the British Type 420) was to be used for both Hedgehog and Mousetrap ammunition, and that the development of the HIR fuze, which was to be interchangeable with the Mark 31, was to be continued under NDRC (that is, by the California Institute group).

The design of the HIR was carried through; about 30,000 of these fuzes were provided for the Navy and went into Service use. Later, the HIR was replaced by an improved version, the HIR III (subsequently designated the Mark 140), which was also developed at the Institute. Still later, a further improved version, the Mark 156, was standardized for Service use.

The purpose of these successive modifications was to increase the reliability and range of fuze functioning — that is, to develop a fuze in which the chances of failure of the arming mechanism would be reduced to the lowest possible minimum, and one which would function on oblique impact angles and against deck gratings and other materials as well as against the metal of a submarine hull.

Throughout this development, which was carried on with a constant interchange of ideas and experiences with the fuze section of the Bureau of Ordnance, the facilities at Morris Dam were in constant use. These facilities, which were set up and operated under the direction of Max Mason, comprised a laboratory for the study of the underwater behavior of projectiles. Instrumentation was devised which gave an accurate record of the sinking rate and path; and in the case of fuze work, arming distance and impact functioning.[1]

Partly as a result of the Mousetrap fuze experience, partly in anticipation of future fuze problems, a special group was organized in 1942 which was to devote itself exclusively to fuze work. This group, headed by R. B. King,[2] was responsible for one of the principal activities of the contract. Their job included not only the development of fuzes for new rockets; they also undertook, from time to time, emergency production of these fuzes for Service use for the Bureau of Ordnance, and they carried on extensive acceptance testing of fuzes produced by the Bureau's prime contractors.

[1]Early in 1944, L. B. Slichter became Cosupervisor with Mason of this activity. Originally he worked under Division C, NDRC; then, during 1942 and 1943, he was Supervisor of a section on antisubmarine applications under the Cal. Tech. contract, OEMsr-418.

[2]On leave from the staff of the Mount Wilson Observatory of the Carnegie Institution of Washington.

## CHAPTER XVIII

# *THE MOUSETRAP GROWS WINGS*

SO FAR, the story of the California Institute's part in antisubmarine warfare has involved only the development of a rocket weapon for surface ships. But the airplane, with its speed and range and hence its ability to search over wide areas, obviously has great potentialities for antisubmarine work. Aircraft were so employed even before Pearl Harbor. But their offensive power was limited. They could not carry enough depth charges for the saturation pattern essential to a successful depth-charge attack. As for bombing, the target was rather small for much chance of a hit. There was the further difficulty that both bombs and depth charges at the moment of release had the forward velocity of the airplane. This meant that they fell along a curved trajectory which varied according to the airplane speed and altitude.

At best, the accuracy was limited to that obtainable by conventional low-altitude bombing methods. In practice the accuracy was not as good as this, for the submarines crash-dived as soon as they were spotted, so the bombardiers had no time to survey their targets, maneuver for the best approaches for attack, or make sighting adjustments. Frequently the bombardiers had as their aiming points only the swirling water where the submarines had dived. A usual report was "Contacted submarine—attacked with bombs and depth charges—observed explosions but no other results."

Although radio and radar equipment were used in addition to vision for contacting surfaced submarines, there was no airborne equipment for locating them while submerged. The net result of these limitations during the early part of the war was that airplanes, though they made many attacks, were useful in antisubmarine warfare chiefly for spotting enemy U-boats and then calling surface craft which could (sometimes) secure underwater-sound contact and make follow-up attacks.

As a means of locating submerged submarines from the air, for following up surface contacts and also for making initial contacts on patrol sweeps, various magnetic methods were developed by British and American scientists. The U.S. Navy found the most promise in the Magnetic Airborne Detector (MAD) developed under Section C-4 of Division C, NDRC. With the search plane flying at low altitude, the MAD could pick up a submarine not too deeply submerged. MAD informed the pilot of the pres-

ence of a submarine, but not until the wings were vertically over it did he know just where it was. From this position he could not attack with any conventional armament.

To meet some of these difficulties, Mason and Slichter, in February 1942, suggested a unique way of using the Mousetrap rocket, then still in its swaddling clothes. Take this rocket being developed for attacks by surface ships on submerged submarines, they said, and fire it rearward from an airplane at a velocity equal to the forward speed of the plane.

This would have the same effect as dropping from a stationary aircraft; the bomb would fall almost straight down to enter the water directly below the release point. The pilot need only hold the plane at a speed to match that of the rocket, and fire when directly over the target, as indicated by MAD or any other means. Or, even better, the rockets could be triggered off automatically by the MAD indication — the vertical trajectory resulting from backward projection was exactly what was needed to match the peculiar vertical location characteristic of the MAD.

Projectiles so used were known (later) as vertical antisubmarine bombs, vertical antisubmarine rockets, retro rockets, or retro bombs. Tentative plans for their development were discussed with Rear Admiral J. A. Furer, Navy Co-ordinator of Research and Development, when he was inspecting the rocket work of the California Institute group early in May 1942. Admiral Furer urged that the development of the retro-firing antisubmarine bomb be continued, and he put the Institute group in touch with Rear Admiral R. S. Holmes, the Commandant of the Eleventh Naval District, in order to provide them with an airplane for experimental purposes. For a time there was difficulty in getting a plane, even though the Bureau of Aeronautics had authorized one; the situation in the Pacific was so critical that none could be spared.

Meanwhile, however, Lauritsen suggested and McMorris worked out an ingenious gadget to demonstrate the general feasibility of retro-firing on the ground without waiting for an airplane. Mousetrap bombs were equipped with two rockets. The first propelled the bomb forward at a velocity of 170 feet per second, the second rocket fired in the reverse direction when the projectile reached the peak of its trajectory. The result, as had been hoped, was that the two velocities canceled each other out, and, as the report phrased it, "the bombs were stopped in mid-air and dropped vertically like dead ducks."

The plane difficulty was solved soon after. By a fortunate chance, Rear Admiral C. A. Pownall, then Commander, Fleet Air West Coast, attended a demonstration of Institute-developed rockets. He was told of the retro-firing project, and it was explained that the initial experimental work could be done in stretches of a few hours at a time, whenever a plane was tem-

porarily available. He saw that a plane was soon provided. Later, as the retro-firing development proceeded, it had the continued support of Fleet Air West Coast, which not only provided planes and pilots but also cooperated in improvements in equipment and in working out tactical procedures.

For preliminary test firing on the desert range, channel-rail launchers like those used for the Mousetrap were satisfactory. But before the projectiles could be retro-fired from a plane in flight, special launchers had to be devised. These must be as simple and light as possible, not too difficult to mount under the wings of airplanes, and designed to have minimum drag. Furthermore, preliminary ground-firing tests must be made to determine whether the plane would suffer any ill effects from the blast from the rocket motor.

Launcher developments became the responsibility of F. C. Lindvall.[1] While range tests of the projectile were being completed, a launcher design was under way, and by late June 1942, everything was ready for test firing from a grounded plane. The plane supplied by ComFair West Coast was PBY-5A (Catalina), chosen because this type of plane was already in extensive use for antisubmarine patrol. The launcher was a set of bent steel channels fastened to the underside of the wing. Attachments on the rockets held them in the channel rails and guided them along the rails when they were fired.

The first ammunition used was the 70-pound Mousetrap projectile (without propellant); this was mounted on the wing launchers with the finned tail at the rear, so that in flight, and in firing, the nose pointed in the direction of motion with respect to the air. In front of the bomb was a separate rocket motor, called a mule because it kicked the bomb backwards tail first and then separated from it.

On June 26, 1942, at the North Island airfield across the bay from San Diego, six rounds were fired from the grounded Catalina. The rounds and launcher functioned successfully, but the blast damage to the wing indicated that additional protection would have to be provided before firings could safely be made from the plane in flight. Blast deflectors were added, and the Catalina was flown inland to the Institute's desert range. (It must have been a queer sight — the amphibious Catalina settling down on the dusty bed of a desert dry lake.)

There, on July 3, 1942, the first retro bombs were successfully fired from

[1]Professor of Electrical and Mechanical Engineering at the Institute (since early fall, 1945, Chairman of the Division of Civil and Mechanical Engineering and Aeronautics). He joined the rocket group late in the fall of 1941 and acted as one of the supervisors of the Launcher Design Section until well into 1943. Early in 1943 a new section, Torpedo Launching, had been set up, of which he became supervisor. This section expanded so far that after 1943 it claimed all his time.

a Catalina in flight.[2] The Catalina was flown by Lieutenant Commander (later Commander) A. E. Hean, who continued to act as pilot and collaborator in subsequent tests.

Events moved rapidly after this first air firing on July 3. The launcher used that day had been a rush job; its main purpose was to demonstrate the feasibility of retro firing. A launcher for Service use would need refinements of design and a larger number of rails. This launcher work was undertaken jointly by the Institute and ComFair West Coast, the former making the launchers and release mechanisms in Pasadena and the latter making the installations at North Island under the supervision of Lieutenant Commander Hean.

The first of the improved launchers consisted of 24 channel rails, 12 fixed to the under side of each wing of the Catalina. The rails were flared outward slightly, and the firing control was arranged so that the projectiles could be fired in sequence in groups of 8 to form a pattern on the surface of the water covering any area 140 feet across the line of flight and 40 feet along it.

While this launcher development was in process, further work was being done on projectiles. It was soon found that the nose-first mounting and the separable motor were unnecessary, because the air velocity of the projectile when leaving the launcher was so low that the tail fins, whether ahead or behind, had little effect on the flight. Consequently, Mousetrap projectiles were used with only slight modifications in further trials. The projectiles were mounted on the launchers in a tail-forward position, and launched backward by their own rocket motors.

The projectile which was finally developed for retro firings[3] had a head of the same external diameter as the Mousetrap round, 7.2 inches, and carried approximately 35 pounds of high explosive. For the motor, more propellant was needed because of the increased weight of the whole round and the necessity of higher velocity for other aircraft. The propellant group developed a tubular three-ridged grain with radial holes for a motor tube 3.25 inches in diameter (an inch more than the Mousetrap motor). This grain was produced in two different lengths, one to give a velocity of 200 feet per second; the other, 300 feet per second. The choice between the two would depend on the type of plane from which the projectile was to be fired or the attack speed. As with the Chemical Warfare rocket and the Mousetrap round, the stabilizing fins at the rear of the motor tube were enclosed by a shroud ring of the same external diameter as the head.

[2]Compare with first air firing of 4.5-inch rocket July 6, 1942 (Page 56 of Chapter X). On the available evidence the retro firing probably represented the first firing of an American rocket from an American plane in flight.

[3]It was variously called the Vertical Antisubmarine Rocket (VAR), the Vertical Antisubmarine Bomb (VASB), the retro-fired antisubmarine bomb, and the retro bomb. For the sake of brevity, the last of these will be used in this account.

Since successful use of the retro bomb would depend upon considerable practice and training with the detection gear, a subcaliber round was also needed. Such a round was developed by modifying subcaliber Mousetrap ammunition in order to match the velocities of the full-size rounds. The subcaliber ammunition was fired from adapter rails which, as in the case of the Mousetrap, were designed so that they could be quickly and easily attached in the channel rails of the standard launcher.

The attack procedure which was being worked out called for the use of flare lights as markers for the retro-bombing attack. That is, the plane would drop a flare when its detection gear signaled a submarine contact. Then a pattern of search sweeps would be flown, with flares dropped on successive contacts, so that the course of the submarine would be marked by the line of flares on the water. When this path was established, then, in a final sweep, the plane would deliver the retro-bomb attack. Hence, the Institute group developed retro flares to match the retro bombs. (These were variously called vertical flare lights, retro flares, or drift signals.) They consisted of the standard flare light, to which a small rocket motor was attached. This motor had an outside diameter of 1.25 inches; the propellant grain was of tubular ballistite, of about 1-inch outside, .53-inch inside diameter. Two different lengths of propellant grain gave velocities of 200 or 300 feet per second, corresponding to those of the two full-size and the two subcaliber retro rockets. In the retro flare, the firing of the propellant initiates a delay train which ignites the flare some 10 to 20 seconds later. During the free fall of the retro rocket a spring mechanism separates motor from flare, and the latter floats on the water, burning for 10 to 15 minutes.

The launcher which was developed for the flare was a tube, partly or wholly retractable into the fuselage of the plane. It could be loaded from within the fuselage; and because the recoil from the small propellant grain was slight, the flare-launching tube could be fired closed-breech, thus eliminating any blast problem.

While all these developments were being carried on, flight testing was continuing both at the Institute's desert range and at San Diego. During that summer the Army Air Forces came into the picture. They had been kept informed about the retro-bombing project. Since Army planes were carrying at that time part of the responsibility for antisubmarine patrol along the Atlantic coast, it was desirable to work out retro-bombing MAD installations for the types of aircraft involved in these operations. The B-18 was the first one selected for experiment. One of these aircraft was flown west to the Santa Monica plant of the Douglas Aircraft Company, which was to design and fabricate a launcher. Their design comprised eight duralumin tubes attached by brackets under each wing.

Flight tests of this equipment made at the desert range revealed two fundamental difficulties. The tubes created an impossible amount of drag;

and the partial vacuum set up in the tubes by the motor blast reduced the velocity of the projectiles about 20 per cent. The launcher tubes were abandoned. They were replaced by 16 bent steel rails of the type already developed for the Catalina. With this launcher equipment the B-18 went through another series of flight tests on the desert range. With a target area 140 feet across and 28 feet long outlined on the ground, and a coil to set up a response in the MAD gear, the B-18 got the following score of hits: 7 out of 22 bombs; 8 out of 10 subcaliber rounds; and 5 out of 9 flares.

Somewhat later, launchers were also engineered for two other types of Army aircraft, the A-20 and the B-24.

While these installations for the Army Air Forces were being developed, the Navy's retro-firing program continued without interruption. A launcher was developed for the TBF (Avenger). Additional work was done with Catalinas, including tests not only on the desert range, to which the Catalinas were frequent visitors, but also with "tame" submarines off San Diego. Experience showed that for maximum results, thorough familiarity with the detection equipment was necessary. But after that familiarity was acquired, the detector plus retro bombing made, as one of the reports put it, ". . . practically a sure-shot combination."

After all this preparation, however, retro bombing was never used so widely as had been anticipated while the development and training work were underway in 1942 and the first half of 1943. The Army was out of the picture by the end of the summer of 1943, as the result of arrangements which gave the Navy sole responsibility for aerial antisubmarine warfare. By this time German submarines had changed their tactics. They were equipped with heavier armament, and, when spotted by a plane, instead of diving they stayed on the surface and opened up with antiaircraft guns. Under these conditions MAD was unnecessary. Conventional armament was better than retro bombs, and their use made the planes too vulnerable to AA fire. Forward-firing rockets (see Chapter XXI) had shown great promise by this time as a weapon for the attack of submarines by airplanes.

Even for the problem of locating and attacking submerged submarines, the Navy did not expand further its MAD-equipped forces, presumably because of the greater promise then apparent in other devices. The retro bomb had been devised to work with MAD; when MAD was displaced, so was the retro bomb. Further orders for retro bombs were canceled (only about 50,000 rounds were produced in all, most of them by the California Institute); and no additional squadrons were trained.

Only two squadrons, both of them flying Catalinas, ever went into operation. The first, VPB-91, trained at San Diego, with personnel from the Institute group helping with the training. This squadron was sent to the South Pacific where it was based at Tulagi. There were few enemy

submarines in that area, and no contacts or attacks were ever reported.

The second squadron, VPB–63, Commander E. O. Wagner, commanding officer, began training at San Diego in the spring of 1943 and then trained further at the Naval Air Station, Quonset Point, Rhode Island. From Quonset, they were sent to the Caribbean, where in further tests they fired inert-loaded rounds at a tame submarine. Then they were ordered to Iceland, where they operated for several months. This happened to be a period of little submarine activity, and again the squadron reported no contacts. Finally they were ordered to Gibraltar, where they were assigned to antisubmarine patrol of the Straits area.

This was a situation in which the combination of special detection gear and retro could function most effectively, since the Straits were so closely patrolled that the only possible way for a submarine to slip through the Straits was to make the run submerged.

On February 24, 1944, two Catalinas of VPB-63 were on patrol off the entrance to the Straits. The detection gear of one of them (No. 15) signaled a submarine contact, and the plane immediately began dropping tracking flares. In the meantime, the second Catalina (No. 14) came up and joined in the chase. Contact was then lost because of a British destroyer, which had come on the scene. After about twenty minutes plane No. 15 regained contact and fired its retro bombs. Explosions from the bombs were reported, indicating that the target had been hit. About two minutes later, plane No. 14 made its attack run, and some of its bombs were seen to explode. Twenty seconds later, the destroyer dropped ten depth charges. Then, after an interval of five minutes, the bow of the U-boat broke the surface at the point of attack, directly in the line with the flares which had been dropped marking its submerged track. The submarine lost all forward way, and settled quickly until only the bow was visible at a 40° angle. One minute later it slid under the water stern first. Meanwhile, a second destroyer had joined the first, and both now attacked with depth charges. After another five or six minutes, the submarine surfaced again and its personnel began abandoning ship without attempting to man the guns. Seven minutes later, a Catalina of another squadron depth-bombed the submarine, obtaining a straddle. Two minutes later, an RAF Catalina got another straddle with depth charges. The submarine then sank, with bow upended, about twenty-five minutes after plane No. 15 of VPB-63 had delivered the first retro-bomb attack.

Considering the number and variety of the participants in this attack, assigning credit for the kill is about as difficult as deciding who killed Cock Robin. From the general viewpoint of winning the war, however, the important thing is that the submarine was sunk. From the viewpoint of this record of rocket development and use, the important thing is that the special detection gear can be credited with making the first contact

with the submarine and the retro-firing gear with making the first two attacks.

On March 16, 1944, three Catalinas of VPB-63 (Nos. 1, 7, and 8) participated in a similar attack. In outline, this episode is much the same. The detection gear of the Catalinas made contact with an underwater object. Contact was lost, then picked up again; and planes No. 1 and No. 8 each delivered a retro-bomb attack. A destroyer standing by reported three explosions from each attack; from the time interval, these were assumed to be hits. The planes reported that immediately after this the U-boat apparently slowed down, and stronger detection-gear signals indicated that it was coming toward the surface. A light film of oil was also seen forming on the water. At the request of the planes, the destroyer then attacked, firing twenty-four Hedgehog projectiles. Other surface craft had come up in the meantime, and one of them fired another twenty-four Hedgehog rounds. Shortly afterward, debris appeared in the water; oil and air bubbles came to the surface; and a small boat sent to investigate picked up "substantial bits of human flesh" and pieces of wood later identified as U-boat locker tops. Under the rigid requirements for assessing the results of anti-submarine attacks, this could be scored only as "probably sunk."

A month later, on May 15, 1944, VPB-63's Catalinas again made a detection-gear contact. Again they delivered a retro-bomb attack; again surface ships joined in the hunt; and again the verdict was a "probably sunk."

Then comes a long period of nearly a year, with no reports of submarine contacts by this squadron, no record of retro-bombing attacks. But finally, during the last few months of the war, German submarine tactics changed again, and attacks were made against English shipping in the English Channel area while the attacking submarines remained submerged. Squadron VPB-63 returned to its special detection-gear and retro-bomb attack. On April 30, 1945 a lone Catalina of this squadron, while flying in the Bay of Biscay area, spotted the *Schnörkel* of a German submarine. No information is available on whether the pilot used his detection gear to trigger his retro-bomb attack, or merely used the *Schnörkel* for a visual aiming point. At any rate, he retro-bombed the submarine, and the wreckage which came to the surface justified still another "probably sunk."

This is the last recorded use of retro-bombing. It brings the whole story of the retro bomb to a neat conclusion, for the probable kill chalked up that April day was also probably the last submarine sunk during the war.

The function of the rocket motor on a retro bomb was to decelerate the bomb from the speed of the aircraft on which it was carried to approximately zero air speed, and thus to secure substantially vertical fall. Another application of the retro-rocket principle was also developed at Cal. Tech. In this, rocket motors mounted on an aircraft torpedo were employed to exert rearward thrust during the fall from the airplane to the water, so

that the torpedo entered the water with a speed about 100 knots less than its speed at the instant of release. In 1942 this looked like an answer to an urgent problem.

The Institute group began its first work on aircraft torpedoes in the fall of 1942. The United States Navy went into World War II with an aircraft torpedo designated the Mark 13. In the period between wars very limited experimental facilities had been available to the torpedo-development group of the Bureau of Ordnance and little experience had been accumulated. Hence, the Mark 13 was very conservatively designed as to plane speed and altitude for release. In combat use, as the volume and accuracy of enemy antiaircraft fire increased, the low altitude and slow plane speed necessary for satisfactory runs resulted in a very high proportion of pilot and plane casualties. If the torpedoes were dropped at higher speed and altitudes, they behaved erratically, "porpoising" and hooking.

A possible solution was proposed in the fall of 1942 by Lieutenant C. C. Callaway, a member of the group from Fleet Air West Coast who, under the command of Lieutenant Commander Hean, were co-operating with the Institute group on the development of the retro-fired antisubmarine bomb. No doubt it was this retro-firing principle which led Lieutenant Callaway, at a meeting at the Institute, to suggest that rockets might be used to decelerate the aircraft torpedo. That is, if the torpedo were fitted with retro-firing rockets, it could be dropped from a plane at high speed, and the rockets, fired during its fall, would decelerate it so that it would enter the water at low-enough velocity to make a satisfactory run.

The idea seemed worth trying, and the Institute group, with NDRC approval, set up a project under W. R. Smythe for developing this rocket application. The following general requirements were set up:

1. The rocket motors must not fire until the torpedo had dropped a safe distance below the plane (10 to 15 feet), so that the plane would suffer no damage from rocket blast.

2. The thrust of the rocket motors must be uniform, so that the minimum amount of yaw would be produced as the torpedo entered the water.

3. Rocket blast must not produce any aerodynamic instability in the torpedo.

4. The torpedo must shed the rocket motors before entering the water, in order to preserve its normal underwater-travel characteristics.

5. The rocket equipment must be adapted to current torpedo installations.

The design which resulted consisted in a belt of twelve retro-firing rockets, with a firing delay so that the propellant grains were not ignited until the torpedo had dropped a safe distance below the plane. Another delay mechanism, which functioned after the motor burning was com-

pleted, disengaged the belt of rocket motors from the torpedo, so that the latter made its water entry free of any encumbrances.

Tests were made with retro-fired torpedoes dropped from a TBF-1C aircraft late in October 1942. The results were not completely satisfactory, but they demonstrated the feasibility of the general design and indicated that a deceleration of about 100 knots could be obtained.

The development was continued well through the following year. There were many bugs to be removed in order to make it reliable enough for Service use. While the process of modification and improvement was still under way, other and far simpler means of solving the problem made it unnecessary to go on with the development of rocket deceleration of the aircraft torpedo. The principal element of this other solution was the addition of a shroud ring to the tail of the Mark 13 torpedo.

The success of the torpedoes so modified marked the fulfillment of a Navy request to NDRC for quickly applicable improvements to the Mark 13 torpedo that would increase its combat effectiveness, particularly by easing the limitations on the speed and altitude of the launching plane. This was a short-term objective of a project for the development of an improved aircraft torpedo. That project had been assigned by NDRC jointly to Divisions 3 and 6, and was carried out largely under their contracts with Cal. Tech. Division 3 was given responsibility for studies of the fundamental hydromechanical phenomena associated with the entry of torpedoes into water; and Division 6 was assigned the job of translating such information into actual designs.

The suggestion that a ring type of tail might improve an aircraft torpedo stemmed from some observations by Mason during the early tests of Mousetrap rounds (which have ring tails) and from other observations made by L. J. Hooper, Professor of Hydraulics at Worcester Polytechnic Institute, working under a subcontract with Cal. Tech. The first studies were made with two-inch models in the High Speed Water Tunnel, which was operated for Division 6 by the Cal. Tech. Hydrodynamics Laboratory under the supervision of R. T. Knapp, Professor of Hydraulics. Then tests were made with full-scale torpedoes launched from a compressed air launching tube by means of which the entry into water could be made under carefully controlled and reproducible conditions that simulated those of a drop from a plane, so that more exact measurements of the behavior of the torpedo could be made than were possible when it was launched from aircraft. Finally, after the design of the shroud ring had been perfected by Lindvall's torpedo-launching section, demonstration tests were carried out at San Diego with torpedoes dropped from Navy planes. Subsequently Cal. Tech. supplied the Navy with 1000 conversion units for the modification of existing Mark 13 torpedoes.

The compressed air torpedo launching station, which was set up especially

for this project, was located along the San Gabriel River, about a half mile above Morris Dam,[4] in a mountain canyon some 17 miles from Pasadena. There the torpedoes were propelled through a 300-foot sloping tube into the river, where their underwater trajectories were traced by means of an array of hydrophones, underwater cameras, and nets. Sometimes the air pressure in the tube was augmented by injecting gases from burning grains of rocket propellant that were ignited by a trip mechanism as the torpedo roared down the tube. In some trials small rocket motors were used to produce pitch and whip of the torpedoes as they hit the water.

In addition to the observations made during these full-scale tube launchings, model-scale studies were carried out with the aid of a glass-sided tank set up on the Institute campus and operated by another Division 3 group. Small-scale models of projectiles were launched into it with a crossbow device. Much was learned in this way about the effect of nose shapes and other characteristics on water entry and on the air bubble in which the projectile rides during its underwater travel. This tank and its associated measuring equipment also furnished similar information about rocket heads that was useful to the designers of aircraft rockets to be used against subsurface targets. (See Chapter XXI.)

[4]The group that operated the launching tube (known at Cal. Tech. as Section VII) was distinct from Cal. Tech.'s Section IV on Underwater Properties of Projectiles that operated a laboratory at Morris Dam itself. This laboratory, set up under the supervision of Max Mason in the early days of NDRC, met the need for full-scale and model study of the phenomena associated with the entry into water of antisubmarine ordnance and of their underwater trajectories. It was used in the development of the Mousetrap (Chapter XVI) as well as in the performance of tests for the Navy of a variety of underwater devices.

CHAPTER XIX

# OLD FAITHFUL

THE DEVELOPMENT of the retro bomb was a high-priority job during the summer of 1942. Early that same summer, the California Institute group began another development, which soon acquired a higher priority than the retro bomb and in terms of wide use and effectiveness made a far greater contribution to winning the war.

This was the 4.5-inch beach barrage rocket. It was generally known as the 4.5 BR. Because of the requests for additional production which kept coming in all during the rest of the war, the production section of the Bureau of Ordnance nicknamed it Old Faithful.

By the summer of 1942, the United States had reached the end of what Fleet Admiral Ernest J. King has called the purely defensive phase of the war. The Battle of Midway in early June ended the threat to Hawaii and the West Coast of the United States, and enabled us to undertake the offensive — a limited offensive at first, but one which steadily grew in power, with results too familiar to need enumerating. Even before the tide had turned moderately in our favor, military authorities could anticipate the general nature of the campaigns which must be waged and could begin to plan the special weapons and equipment which would be needed.

It was clear that amphibious operations would play a large part as the United States and her allies assumed the offensive. The invasion of North Africa, which was being planned as early as July 1942, would obviously be such an operation; so would any later assault on continental Europe. In the Pacific, for the recovery of island positions from the Japanese, and General MacArthur's island-hopping and end-running up the coast of New Guinea and on into the Philippines, amphibious operations would constitute the backbone of the whole scheme of attacks.

The provision of special weapons and equipment for amphibious operations was particularly the concern of the Navy, because it was the Navy's responsibility to prepare the way for the landings and to get the troops ashore — and in many cases, the troops would be elements of the Navy's own Marine Corps. It is not surprising that it was a Navy officer who suggested the development of a special type of rocket projectile for amphibious operations.

This suggestion was made by Vice-Admiral Wilson Brown, Commander Amphibious Forces, Pacific Fleet, at a demonstration of the Mousetrap

and other California Institute rockets. He had assumed that command only a short time before, having previously been the commanding officer of the task force which had made the brilliant raid on Lae and Salamaua some months before. No doubt when he witnessed the rocket demonstration he was thinking of the necessities of his new command, and therefore saw in rockets an opportunity to take care of one of the most critical phases of an amphibious operation.

This critical phase is the interval between the lifting of the preparatory naval gunfire barrage and the landing of the first wave of attacking troops on the beach objective. No naval gunfire barrage, however concentrated, can be depended on to eliminate all the defenders. But naval gunfire must stop as the first landing wave approaches the beach; and the interval gives the surviving defenders a chance, as at Tarawa, to organize and pour devastating fire into the landing craft. Something was needed to continue the barrage fire as the landing wave came in, and so reduce as much as possible the crucial interval after naval gunfire barrage (and preparatory air strikes) had stopped.

What Admiral Brown suggested was a rocket projectile with a range of about a thousand yards with a light-case head for maximum fragmentation and anti-personnel effect, and launchers that could be mounted on the landing craft that took the troops in or on light support craft that could go in with or just precede the first landing wave.

The California Institute group undertook this development at once. The long hours in which they had sweated out motor design now paid an extra dividend: the motor already developed for the Mousetrap projectile proved to be satisfactory, with only minor changes, for the barrage rocket. Some preliminary studies showed that standard 4.5-inch steel tubing, if shaped for the head, would give good fragmentation. This somewhat eased the procurement and production problem. The head which was worked from this tubing was, in effect, a 20-pound general-purpose bomb. It was about 13 inches in length and carried 6½ pounds of TNT. An adapter screwed into the base of the bomb provided a means of attaching the motor. Because of the lower weight of the head (20 pounds as against the 55 pounds of the Mousetrap projectile), the Mousetrap motor provided the desired range, with, in fact, about a 10 per cent bonus over the thousand yards originally suggested.

Development of the barrage rocket proceeded very rapidly and the first models were test-fired satisfactorily on the Institute's desert range. Concurrently with the development of the projectile, the design of a suitable launcher was carried forward. Since it had been decided that the first installations were to be made on the armored support boat, the available space and special requirements of this craft conditioned the dimensions, etc., of the launcher. Thus, the launcher rails were kept to a length of 5

feet, and the height and width were limited so as not to interfere with the handling of the boat from davits or in the use of the machine gun with which the boat was equipped. The final design was a cratelike arrangement holding 12 BR's in 4 tiers of rails, each tier having launching rails for 3 projectiles. The support-boat installation consisted of two of these launchers, mounted to starboard and port on the coaming and situated so that they could be loaded from the armored cockpit.

The launcher was fixed in azimuth parallel to the centerline of the boat, but its mounting permitted any angle of elevation from 0° to 45°. As with the Mousetrap, the launcher was aimed by aiming the boat; range was controlled, in practice, by maintaining the proper distance between the boat and the target area.

The first launcher was fired on the desert range as part of a comprehensive test of projectiles and related equipment, again with generally satisfactory results. Two days later, the launcher was taken to the Destroyer Base at San Diego and mounted on a 37-foot support boat, for a full-scale sea test. Thirty-six rounds of ammunition, brought up to a standard weight, but with inert heads, were fired from this support boat at a target on the beach at Northwest Harbor, San Clemente Island. Again the results were generally good. Loading and elevating the launcher with the boat underway "seemed very easy to all hands." The location of the launcher was satisfactory, and the armor baffles of the boat provided sufficient blast protection for the crew. Only two difficulties appeared. In one salvo, the rockets were fired too rapidly and interfered with each other in flight. This could be corrected by training the firing member of the crew to space the rounds at longer intervals; but as a surer correction, the Institute group undertook to provide firing switches adjusted for the proper time intervals.

The second difficulty was that no available range finder appeared to be satisfactory for use on a small boat. This indicated the necessity of preliminary ranging shots. (Later, a subcaliber round for ranging shots and training was developed, but never went into use. With the barrage rocket, the training problem was very much simpler than with the Mousetrap and the retro-fired antisubmarine bomb. And as the tendency later was to accumulate greater and greater fire power of barrage rockets, a few preliminary rounds could well be expended for getting the range.)

While the launcher development was proceeding, further work was being done on the projectiles. The principal problem was fuzing. In the earlier models, a pressure-arming base fuze was used, which held in the adapter connecting the head and the motor. This fuze was armed by the admission of gases from the motor tube; these gases built up pressure to the point where it collapsed a diaphragm; this, in turn, actuated the arming mechanism; and the fuze then fired on impact. For barrage-rocket pur-

poses, however, a nose fuze is more suitable, as it gives instantaneous detonation and hence more effective fragmentation. The problem was to provide a nose fuze for the BR. Such a fuze was developed by modifying a trench mortar fuze. In its original form, this fuze depends upon setback to initiate the arming process and the freeing of a detent as the shell leaves the mortar. The essential modification consisted in adding a vane to the fuze. Setback frees this so that it rotates during the flight of the rocket and in turn frees the detonator pin to function on impact. This was an AIR fuze (i.e., Air-arming, Impact-detonating Rocket fuze). Later, the fuze section of the Institute group developed a new AIR fuze, incorporating additional safety features.

With the launcher development completed and the barrage rocket freed of bugs,[1] the focus of activity shifted for a while to the East Coast. A demonstration had been requested for the Commander, Amphibious Forces, Atlantic Fleet. California Institute personnel went with crate launchers for two support boats, and arranged for a supply of ammunition. After some preliminaries involving installation and crew training, the demonstration was held August 25 in Chesapeake Bay, at Cedar Point, Solomon's Island, Maryland. In addition to Navy representatives, Army and Marine Corps officers and NDRC officials were present.

The success of the demonstration is indicated by the action that was taken the following day, when the Bureau of Ordnance requested that the California Institute supply, for Amphibious Forces, Atlantic Fleet, 50 launchers (that is, 25 pairs, each pair consisting of 1 port and 1 starboard launcher), 3000 rounds of 4.5-inch barrage rockets, and 3000 fuzes, all to be delivered within 30 days.

The Institute group, as set up under NDRC, was primarily an organization for research and development. Production of weapons for Service use had not been contemplated as among its functions. The expectation was that when a new weapon had been developed to the point where one of the Services could put it into the field, production would be taken care of by prime contracts between industrial producers and the Navy Bureau of Ordnance or the Army Ordnance Department. In the case of the BR's, fuzes, and launchers, however, prime contracts could not possibly be made and industrial production established within the thirty days allowed. The urgency of the request implied pretty clearly that the barrage rockets were needed for an amphibious operation already scheduled, and the Navy believed that their use would enable that operation to be carried out with a small expenditure of American lives. Since the Institute was already pro-

[1]No weapon is ever developed to the point where it is completely freed of bugs. A compromise has to be made between the ideal of a nearly perfect weapon, which is to be achieved at some indefinite time in the future, and the necessity for a workable weapon which can be put into Service use in the present.

curing limited numbers of BR components and launchers from its own subcontractors, the chances seemed good that by increasing these subcontracts the Navy's requirements could be met.

Because of these considerations, NDRC and OSRD authorized the Institute group to go ahead. Additional inspection and assembly facilities were improvised in Eaton Canyon. With the Navy's help in handling priorities problems, the Institute procured the materials, distributed them to subcontractors for processing, collected them, and did the assembling, inspecting, painting, and motor loading. Practically the entire research staff pitched in as expediters and inspectors; workers doubled shifts, and regular hours were disregarded. The Navy took care of the loading of heads with high explosives. These rockets utilized a modified Army M-52 trench mortar fuze, and the cross-country journeys of the fuzes for unloading, modification, and reloading would make a saga in themselves. It was a hectic time, but by October 10 the last of the rockets were finished and flown east to meet the deadline.

Later, the reason for the urgency became clear. The barrage rockets produced during that strenuous month were used in the Casablanca landing, when the assault of North Africa began on November 8, 1942. This was only 70 days after the Bureau of Ordnance's crash production request.

The Navy, soon after asking for the first 3000 rounds, set up a barrage-rocket training program. L. A. Richards[2] of the Institute's launcher section was loaned to the training center at Bloodsworth, Maryland, to assist in setting up the training program and getting it under way, and to prepare a training manual. By the end of September, the manual was ready, fifteen 5-man crews and one 8-man crew had been trained; and a steady flow of trainees had been established.

The crash production of 3000 barrage rockets was only the beginning of a long series of production commitments. On September 30, 1942, the Bureau of Ordnance requested another 3000 rounds and fuzes, plus 50 launchers, for use in the Pacific. These were turned out during October and November. On December 30, the Bureau requested 5000 BR's. On February 8, 1943, the Bureau asked for 17,000 more, with a March 30 deadline, and added, "The need for these additional rockets is very urgent and the Commander in Chief, U.S. Fleet, has directed that every effort be made to obtain them in the shortest possible time."

On May Day, 1943, came a request for 70,000 propellant grains, to be delivered at a minimum rate of 20,000 per month, beginning the first of June. The Bureau of Ordnance's prime-contract production was expected to come in by late spring 1943, but various complications prevented it

[2]A member of the scientific staff of the Regional Salinity Laboratory of the United States Department of Agriculture, Riverside, California. He took leave of absence from the Laboratory early in the summer of 1942 in order to join the Rocket group at the Institute.

from reaching its anticipated volume until considerably later. In the meantime further needs developed for Service rounds. The result was that the Institute continued production of barrage rockets through the rest of 1943 and into 1944. By the end of the war, the total U.S. production of 4.5-inch barrage rockets had reached a total of over 1,600,000 rounds. Compared to the number the Bureau's prime contractors could turn out once their production lines were going, the California Institute production was only a trickle. But it was a significant trickle, for during a considerable period it was the only source of barrage rockets.[3]

Further development work on barrage rockets included a faster-burning propellant grain to decrease dispersion, a subcaliber ranging round, which never went into Service use, and special heads for smoke and incendiary filler. The Bureau of Ordnance, early in 1943, saw the need of smoke rockets to lay screens ahead of landing craft. Since this need was urgent, the Institute group was asked to work out the modification and manufacture of 250 heads for testing, and loaded several thousand additional heads during the summer. The desirability of designing special smoke heads was discussed with the Chemical Warfare Service, which had cognizance over the development of chemical warfare material for both Army and Navy, and the Institute group began work on a head which included a tetryl booster for greater dispersion. This head proved superior for laying smoke screens over both land and water.

These early smoke heads were filled with FS liquid smoke (i.e., "Fuming Sulfur," also frequently designated "Fuming Stuff," a mixture of sulphur trioxide and chlorosulphonic acid). This was later abandoned for white phosphorus filling, which experiments showed to have an anti-personnel and incendiary value, to be longer-burning, and to be only slightly less effective for smoke screens. The last stage in the development of smoke heads takes us beyond the Institute's activities.

The story of the Institute group's work on barrage rockets in 1943 is very largely the story of launcher design and development. Some of these launchers were devised to meet definite requirements; others, to anticipate special uses to which the barrage rocket might be put.

The 12-round "crate" for support boats was the first launcher developed in connection with the 4.5-inch barrage rocket. The next was a launcher for the 2½ ton 6 x 6 amphibious truck, DUKW (commonly referred to as the Duck). This vehicle had been developed under the supervision of Division 12, NDRC. Ducks seemed to be so well suited to the needs of the Second Engineer Special Brigade (2nd ESB) that a flock of them was assigned to that command, which during the latter part of 1942 was completing its

[3]Since the OSRD funds which financed the Institute group were designated only for research and development, barrage-rocket production by the Institute and similar crash production of other rockets later were taken care of by transfers of Navy funds to OSRD.

training at Fort Ord, California. It was believed that barrage rockets would be a valuable addition to the 2nd ESB's ordnance for their prospective amphibious operations.

Early in November, Palmer C. Putnam, a Division 12 Technical Aide, who led the division's development of DUKW and was working with the 2nd ESB on its uses, arranged to have the California Institute group engineer a rocket launcher installation. A Duck was to be sent south to Pasadena for this purpose as soon as the 2nd ESB's complement arrived at Fort Ord.

Time passed; the Ducks were delayed and did not arrive until shortly after the middle of January. Fowler hurried up to Fort Ord, met Lindvall who was already in that region on other rocket business, and the two of them drove a Duck nonstop back to Pasadena, arriving in time for work on the launcher installation to begin on the morning of January 19, 1943.

In the meantime, the departure date of the 2nd ESB had been advanced. There was no time to design and develop a special launcher for the Duck. The best that could be done was to improvise an installation from elements already available, so that the 2nd ESB and its commanding officer, Brigadier General William F. Heavey, could get some idea of the possibility of the Duck as a rocket vehicle. Such an installation was sketched out. It consisted of three of the crate launchers (the kind already developed for support boats). They were mounted side by side across the cargo space, which was sheathed with sheet metal for protection against rocket blast.

By working straight through from the morning of the 19th, the Astrophysics Machine Shop on the campus finished the job at 8 P.M. on the evening of the 20th. The credit for rushing it through goes to Frank Fredericks, who expedited this as he did many other emergency jobs for which the rocket group called on the Astrophysics Machine Shop.

Early on the morning of the 21st the Institute caravan started north: one new-fledged rocket Duck and one ammunition truck. The latter was driven by Bowen. He was serving in a dual capacity. As a representative of the photographic section, he was going to take pictures of the demonstration. As a member of the Contractor's Safety Committee, he was going in order to allay the apprehensions of that Committee. The demonstration was to include the firing of BR's with live heads (i.e., loaded with high explosive). The Safety Committee was very dubious about transporting live ammunition and gave their hesitating consent only when Bowen, as a member of the Committee, volunteered to drive the truck himself. The other members of the party were Fowler, Lindvall, and Gould.[4]

The demonstration firings were conducted at Fort Ord during the morning and afternoon of the 22nd, and the morning of the 23rd. In all,

[4]Albert S. Gould, an electrical engineer by profession, had come to Pasadena from Phoenix, Arizona, in the summer of 1942, to join the Launcher Section of the Rocket group. Eventually, he headed a subsection on special launcher problems.

120 rounds of BR's were fired. General Heavey and his staff were enthusiastic about the results — so enthusiastic, in fact, that they refused to part with the demonstration Duck, though the three-crate launcher installation was admittedly only a stopgap. And when the 2nd ESB sailed out of the port of embarkation, they took with them 2000 BR's and 36 additional crate launchers, which the Navy had released to them from supplies on hand at the Naval Ammunition Depot, Fallbrook, California.

In return for the demonstration Duck, General Heavey released two Ducks to the California Institute, to be taken back to Pasadena and used in the development of a launcher designed to make better use of the Duck's rocket-carrying capacity. Four of the new launchers were to be sent overseas to the 2nd ESB as soon as possible.

This first chapter of the rocket Duck story has been told in some detail, because it illustrates the kind of emergency that arose frequently, when members of the launcher group had to work under forced draft to meet a deadline for a new development.

With the two Ducks back in Pasadena, work began immediately on a 144-round launcher. It is unnecessary to follow the development in detail. It was first tested on February 4, 1943; the usual stages of improvement and further testing followed until March 18, when the launcher was fired from a seaborne Duck in San Diego Bay with completely satisfactory results.[5]

While this work was in progress, engineers from the Coach and Truck Division (later the Yellow Truck Division) of the General Motors Corporation came West to inspect the launcher with an eye to putting it into production. They were brought into the picture by Division 12, NDRC. This division of General Motors had worked under contract with Division 12 in developing the Duck and was to continue that association for the purpose of launcher procurement.

The visiting engineers suggested a different design, one which consisted essentially of several racks of launching tubes. The 144-round launcher developed by the Institute group was built as a unit; hoisting apparatus was necessary to get it into or out of the Duck. With the suggested General Motors design, each rack of tubes could be installed or demounted separately and hence would be light enough to be handled by two men. Furthermore, the launching tubes involved in this design were better adapted to General Motors' equipment for and experience in sheet-metal working.

General Motors was told to go ahead. The first version was brought West for a test at San Diego. Like the Institute launcher, this was designed to hold 144 barrage rockets. It consisted of 12 units or subassem-

[5]Another version of this same launcher, only slightly modified, was designed for mounting in a 2½-ton 6 x 6 truck.

blies, each of which was made up of a rack of 12 tubes or barrels 5 feet long, fixed in line fore and aft and inclined forward at a firing angle of 45°. The tubes vented into a blast channel which formed the bottom of the rack.

As with all other launchers, the first model needed modifications, and after each modification there had to be another test firing. Minor changes could be made in the Institute shops; twice, for major rebuilding, the launcher was sent back to the Truck and Coach Division shops at Pontiac, Michigan. Finally the launcher passed its firing test with flying colors, when Gould fired an uninterrupted salvo of 60 barrage rockets with high-explosive heads. This performance satisfactorily concluded the tests of the BR launcher for the Duck.

The launcher which had evolved through this cut-and-try process had 10 racks of tubes instead of the 12 originally planned, giving the Duck a load of 120 rockets. The GMC-designed fire-control box, located in the cab and operated by the codriver, included a motor-driven selector switch with 120 contact points. When the firing button was kept depressed, these contact points were closed at ½-second intervals, so that a complete salvo of 120 rounds could be fired in one minute. The control box also permitted firing single rounds.

Since the launchers were fixed in position, aiming was accomplished by aiming the Duck. In firing, the standard practice was to use a few single shots to get the range, and then fire the remaining rounds, or as many of them as desired, in continuous salvo.

The four launchers promised to the 2nd ESB were fabricated by the Truck and Coach Division of General Motors during July and were shipped overseas early in August 1943.[6]

A miscellaneous array of launchers developed during 1943 for the barrage rocket can be sketched briefly. They were developed for a variety of reasons: pure speculation, for instance, based on a guess that a need might eventuate; or in response to a requirement which was later canceled. One or two served a useful purpose for a while but were superseded by something better.

Early in 1943, a 10-round launcher was also developed for the land or amphibious jeep. This consisted of a corrugated steel sheet with ten 8-foot

[6]In the fall of 1943, Headquarters, Army Service Forces, was called on to provide the Mediterranean Theater of Operations with ten rocket Ducks. Arrangements were made with NDRC to procure these from GMC through the NDRC Rocket group at the California Institute. The Institute also assumed responsibility for inspecting the launchers at the factory and for training Army personnel in their use at Camp Gordon Johnston, Carrabelle, Florida. The first launcher was sent to the Engineer Amphibian Command, Camp Edwards, Massachusetts, and from there overseas. Two launchers were sent to Camp Gordon Johnston, where Gould trained and demonstrated from November 10 to 23. The other seven launchers were shipped to the New York Port of Embarkation on November 23, 1943. Whether any of this material ever went into Service operation is not known.

channel rails and auxiliary upper rails to hold the rounds in position while the vehicle was in transit. With a pivot mounting at the back of the vehicle, the rails, which projected forward over the driver's seat, could be given any elevation between 5° and 45°.

One group of launchers had been designed with light weight and portability as the principal feature. One of these was a light rail for a single round, supported by a folding bipod at the front. A variant of this, designed for greater compactness, featured a short rail with a telescoping extension. The single-rail launcher was used at least once by the Marines on Bougainville. But the BR has too great dispersion for single-shot effectiveness; and bringing up enough single rails and rockets for area bombardment involves too many troops.

Other lightweight launchers were variations of the packing-box idea. Made of wood, they were large enough to hold two or three rounds. When the box was opened, the cover could be readjusted as a simple three-rail launcher. The idea was that the launcher would be expended in the firing of a few rounds. This type of launcher was sometimes mounted on the stern of a support boat used as a landing-wave director, and might fire ranging shots for other craft with greater rocket fire power.

One type of packing-case launcher was built sturdily enough so that it would survive a parachute drop. Considerable work was also done on packaging rounds and on a light, single-rail launcher for parachute drop as equipment for Marine paratroopers. But shortly after these developments were completed, the word came that Marine Corps troops would not be used in parachute operations.

To provide more rocket fire for landing operations, a launcher was developed to carry 88 barrage rockets on LCM's. This launcher consisted of paired rails, a set of 22 such pairs being mounted on either side of the LCM. This equipment saw considerable Service use, but was gradually replaced by the 12-round automatic, which deserves a section to itself.

The next development was the 12-round gravity-feed launcher for the 4.5-inch barrage rocket, designated "Rocket Launcher, Mark 7, Experimental" by the Bureau of Ordnance and nicknamed Sandy Andy from the familiar toy which first suggested it.

The idea of the 12-round automatic was conceived by Richards. Although his colleagues, at the start, were far from enthusiastic over the idea, he persisted. The modifications required after the first model was test-fired were not many, or difficult; and the launcher was soon ready for use.

The gravity-feed launcher is very simple. It consists of a pipe frame supported by a base plate. The frame holds 12 barrage rockets in 2 vertical tiers side by side. There are 7 rockets in the firing tier. The bottom rocket rests on a rail, with the shroud rings which surround the fins resting against knife-edge contacts. The second rocket is resting on a reel; and

when the bottom rocket fires, the reel lets the second down onto the rail, and the third drops into the reel. So the firing and feeding go automatically. A swing gate, held in the closed position by the rockets in the first tier, is finally released. The 5 rockets in the second tier begin dropping into the reel and are fed over to the rail and the firing contacts. The whole launcherful of rounds can be fired in about 40 seconds. The pipe frame is pivoted to the base plate so that it can be lowered for transport and storage, and adjusted for firing to any elevation between 30° and 45°.

This launcher embodies several distinct advantages:

1. A simple wiring arrangement for single-round or salvo firing. (Other multiple-round launchers require an electrical lead for each rail or tube — 12 for the crate, for instance; 120 for the Duck.)
2. Greater safety for the operator. (This can be loaded from the side. Most other launchers have to be loaded from the muzzle or the breech; and in case of an accidental firing, a position beside the rocket is a good deal safer than one in front of it or in the blast area at the rear.)
3. Adaptability to a great variety of installations.
4. Low ratio of launcher weight to ammunition. (The launcher weight is only a little over 100 pounds; hence, the launcher weight per round is about 8 pounds.)

There is only one disadvantage to the gravity-feed automatic. A misfire will leave the offending round on the firing rails and hence all the rest of the rockets in the launcher will be hung up. This drawback has to be accepted as inherent in the design. The remedy lies in removing the two causes of misfires; faults in the wiring and imperfections in the rounds. The first can be taken care of by careful maintenance; the second, by careful inspection on the part of manufacturers.

Bureau of Ordnance engineers, when they first saw the gravity-feed BR launcher, were dubious about accepting the design for Service use. They disliked the possibility of a misfire, "hanging" the remaining rounds in the launcher; and they considered the base plate too small and the frame too high for stability. So, although the Bureau legitimized the launcher by giving it a mark number, they hedged by designating it "experimental." And by the middle of 1944, they had proved 20,000.[7]

The gravity-feed automatic proved to be the most versatile and widely used of all the barrage-rocket launchers. It could be mounted on practically anything — and was. A simple attachment allowed two to be hung outboard on a jeep. With light blast scoops, twelve could be mounted in the

[7]The first procurement was a crash-production job, carried through splendidly by the Joshua Hendy Iron Works, Sunnyvale, California, in spite of an apparently impossible deadline and great difficulties in getting the necessary material.

cargo space of a Duck or a 2½-ton truck. These launchers were mounted on ¾-ton 4 x 4 and 1½-ton 6 x 6 trucks. They were mounted on LCV(P)'s, on LCM's, on LCS's, on LCS(L)(3)'s. They were mounted on PT boats and on LVT(A)'s — the latter the combat version of the amphibious tractor commonly known as the Buffalo. They could have been mounted on rowboats had there been any reason to do so.

In the Pacific, LCI's, with automatic launchers mounted on every available foot of deck and coaming space, came to be used as the standard rocket gunboat for all sizable amphibious landings.

The wide and successful use of this launcher, and the respect which it won from its users have certainly justified the faith and persistence of its originator.

This is not a Navy or Army history; it cannot present an extensive record of the performance of any OSRD weapon; but after all, the justification for research and development, under forced draft, frenzy of crash production, and large-scale Navy procurement, lies in the use of the weapon. There is no confusion in the record about the warm spot which the beach barrage rocket enjoyed in the hearts of amphibious forces in both oceans.

We have already spoken of the crash program which yielded barrage rockets for the Casablanca operation, fired from 12-round crate launchers mounted on support boats. Subsequently they were used in every major landing in the European theater. At Salerno, they silenced enemy gunfire directed from the beach at the landing waves. In the invasion of Southern France, the LCS(S)'s went in with their launchers loaded with high-explosive heads and carrying a reload of smoke rockets. At H minus 5 minutes they fired at targets of opportunity, then reloaded and used the smoke rockets, as the report says, "with good results." [8] But they were even more ubiquitous in the Pacific.

Honor of firing the 4.5-inch barrage rocket first against the Japanese was held by the Second Engineer Special Brigade, which, it will be recalled, had left for Australia early in 1943 with an improvised rocket Duck, mounting three 12-round crate launchers, 2000 rounds of barrage rockets, thirty-six additional crate launchers, and the promise of four bigger and better ones which were to be designed and built as rapidly as possible.

After further training in Australia they leap-frogged along the northern

[8] At Anzio, the Normandy landings, and the invasion of Southern France, American rocket craft were supplemented by British LCT's (approximately twice the size of American LCI's) which had been converted for rocket firing. The British rockets which they fired had 5-inch heads and a range of about 3500 yards. One type of LCT carried 800 rockets in launchers and one complete reload; the other, about 1000 rockets. The complete salvo could be fired in 90 seconds; four to five hours were required to get the reload in the launchers. At the Quebec Conference in August 1943 it was decided that a number of these British LCT's should be turned over to the United States Navy. Of the thirty rocket LCT's which took part in the landing operations in Southern France, fourteen were manned by United States Navy personnel.

coast of New Guinea with General MacArthur. Finschhafen was captured October 2, 1943, but fighting continued around it until Satelberg, a strong point fifteen miles inland, was captured about a month later. The 2nd ESB took part in this fight for Satelberg and first used their rockets not amphibiously but on land.

Major (later Lieutenant Colonel) Charles K. Lane and Lieutenant Vermell A. Beck mounted crate launchers on a ¾-ton weapons carrier. With the help of Sergeant Chandler Axtell, they got the weapons carrier several miles through the jungle and up a mountain trail to a point where they could fire their rockets on Satelberg. The rocket fire took the Japanese completely by surprise. They let loose with all their artillery, but since they didn't know where the hostile fire was coming from, they succeeded only in revealing their artillery positions to Australian artillery fire and Matilda tanks, the noise of whose advance on the Japanese pillboxes was drowned out by the roar of exploding rockets. This was the first land use of American rockets in any of the Pacific areas.

The 2nd ESB also claims the first use of rockets in a combat landing in the Pacific. This was in the Arawe landing of December 15 where, after preliminary naval bombardment and a strike by a squadron of B-25's, two of the 2nd ESB's rocket Ducks (with the new 120-barrel launchers) under the command of First Lieutenant (later Captain) Walter D. Beaver, laid a barrage on the beach to cover assault waves of LVT's (commonly called Buffaloes). For the next six months, the brigade's rocket Ducks had an important part in nearly every amphibious landing operation which the 2nd ESB carried out, until, worn out, they were replaced by the more useful combat Buffalo. The Buffalo, it is true, carried fewer rockets than the Duck — the first rocket Buffaloes mounted two crate launchers; later versions mounted four gravity-feed automatic launchers. But the Buffalo, being tracked, could negotiate reefs and potholes and muddy terrain where the Duck was hung up.

Landing operations, however, tell only a part of the story of the usefulness of the rocket Duck and later the rocket Buffalo. Sometimes they went on inland with the assault troops. Their job was not only to get the infantry ashore but to see that they stayed there. In these subsidiary operations, the rocket amphibians, Ducks and Buffaloes, frequently were used as light tanks. In the Cape Gloucester landing, for instance, Marines on Yellow Beach were halted by a Japanese pillbox "at a most strategic road junction" until a rocket Duck was brought into range and disposed of it with twenty rockets.

From the original four rocket Ducks, the 2nd ESB, as it got to know the usefulness of barrage rockets, steadily increased the rocket power of its Provisional Support Battery. The Ordnance Officer, Major (later Lieutenant

Colonel) Elmer Volgenau in a "Comment on Support Battery Doctrine," enumerated the requirements of amphibian artillery in destroying and neutralizing enemy beach installations and then added, "The answer to these requirements as far as an ESB is concerned is the tactically and strategically sound, skillfully employed 4.5 Barrage Rocket. This rocket gives a multiplicity of applications varied enough for any problem either of attack or defense which may be encountered." By the end of the war, the 2nd ESB's Provisional Support Battery had a fire power of 3744 rockets without reloading, from launchers mounted on 6 Flak LCM's, 6 Rocket LCM's (each of which carried forty 12-round gravity-feed automatic launchers) and 12 rocket Buffaloes.

Late in 1943, the Commander, Motor Torpedo Boat Squadrons (MTBRons), SEVENTH Fleet, having heard about the 2nd ESB's use of barrage rockets, sent his gunnery officer to 2nd ESB headquarters at Oro Bay, New Guinea, to investigate. At that time the use of barrage rockets on motor torpedo boats (PT boats) had not been authorized by the Navy in Washington. This was understandable, for the high dispersion of the barrage rocket would seem to make it unsuitable for the usual PT mission.

From the 2nd ESB, MTBRons SEVENTH Fleet secured 200 barrage rockets on loan and two 12-round crate launchers by release arranged through Navy channels. The launchers were mounted on one of the PT boats of Squadron 24 and were first fired in the action at Cape Coiselles. The results were so good that, through the efforts of one of the officers involved, all the PT boats of Squadron Seven were equipped with launchers.

They took part with great success, in the Aitape landing; in a short time during that operation they knocked off 107 enemy craft of various kinds. Most of the boats in Squadron Eight were also equipped with launchers; and by the end of the Aitape operation there were one or two PT boats with launchers in every squadron. Eventually there was a total of about 60 PT's so equipped in MTBRons SEVENTH Fleet. Some mounted the 12-round crate launchers; others, the 12-round gravity-feed automatic, as the latter became available. In either case, each boat carried two launchers, mounted starboard and port, forward of the torpedo tubes. To protect the deck from rocket blast, improvised blast shields were used until the spring of 1944; after that, the Bureau of Ordnance's blast deflector adapter kit for the automatic launcher was available. Standard practice was to send each PT boat out with 48 rounds of rockets.

In addition to providing additional rocket fire in landing operations, PT boats were principally used to harass enemy coastwide shipping. On numerous occasions, a patrol would eliminate one or two enemy barges; once, two PT boats disposed of 7 barges on one patrol. Off Ormoc, in December 1944, they sank a Japanese lugger, which was about 130 feet long, with rockets

alone: this is believed to have been the first unassisted rocket sinking. (Usually, fire from automatic weapons was used along with the rockets.)

When one of the officers of MTBRons SEVENTH Fleet was back in the United States in the spring of 1945, he was asked whether the high dispersion of the barrage rockets didn't make them a rather ineffective weapon against enemy barges. "No," he answered, "because it doesn't make much difference whether we get a direct hit or not. The Japs have got so that as soon as we open up with rockets, they all go overboard. Then we can come up alongside and sink the barge with automatic weapon fire, or chop a hole in the bottom with a hatchet."

By the end of 1944, MTBRons SEVENTH Fleet were using 2500 to 3000 rockets per month and were well satisfied with the results they were getting. And up to January 1945 they had not had a single accident or casualty that could be attributed to rockets. Beginning at Cape Coiselles, the rocket-equipped PT boats took part in all the operations along the New Guinea coast and on through the Philippines campaign.[9]

In the last few months of the war, the barrage-rocket equipment of PT boats began to be replaced by a new launcher designed for a more effective rocket. But the story of why and how this development came about belongs in a later chapter.

While the second Engineer Special Brigade, in the Satelberg action in October 1943, had the distinction of first firing the barrage rocket as a land weapon, the Marines made the most extensive land use of it. Early small uses of a single rail launcher at Choiseul and Bougainville did not impress Marine Corps Headquarters.

Interest was revived, however, in preparations for the Marianas. The 1st and 2nd Provisional Rocket Detachments formed and trained at Oahu, attached to the 4th and 2nd Marine Divisions, saw repeated action on Saipan and Tinian; the action report of the 4th Marine Division devotes a full page to recommendations of their efficiency. But the rocket detachments remained "provisional."

Even after Iwo Jima, where the rocket detachments again saw action, the order went out from HQ to do away with them. This order was rescinded on field request so that they could be used in the conquest of Okinawa. The results there were so good that Headquarters reversed itself and decided that some type of rocket unit should have a place in the permanent table of organization.

By the end of the war, each Marine Division had a rocket detachment with the same organization as the provisional detachments, three officers and fifty-three men. The rocket equipment consisted of twelve 1-ton trucks,

[9]Some PT boats in the Mediterranean, after the fall of 1943, also had the 12-round gravity-feed launchers for barrage rockets.

each mounting three 12-round gravity-feed automatic launchers (plus some other installations made in the field, such as pairs of the same type of launcher hung outboard of a jeep.[10])

The Marine rocket detachments, as they functioned on Saipan and Tinian, Iwo Jima and Okinawa, were used to supply concentrations of fire for special needs. When infantry advance was held up by a local Japanese strong point, or when local concentrations of enemy troops needed to be broken up and demoralized, the rocket detachment would be called on. The target would be assigned and identified; the detachment, or as much of it as the occasion required, would go into action. Since the launchers were truck- or jeep-mounted, they could be maneuvered into position quickly, deliver their fire, and then, if necessary, get away before counterfire could be brought to bear on them. In this way they provided a highly mobile supplement to regular artillery. The verdict on them is summed up in an officially conservative statement: "The rockets are very popular with the various combat units because of the effective support they provide."

The uses of barrage rockets already described are interesting examples of field resourcefulness or the versatility of the rocket. But its main use through the war was, of course, to lay down a massive barrage covering the first landing wave in amphibious assaults and to clear the beach of enemy personnel and obstacles. The vast majority of the barrage rockets produced during the war were used for this purpose.

The first craft to be equipped with barrage-rocket launchers was a small support boat. The continuing tendency was, however, to seek area saturation by providing more fire power. This meant launcher installations for larger craft and using more of the craft. Some of these launcher installations were worked out in the States; others were developed in forward areas, where additional training programs were also carried on. For example, the first LCI's (157-foot Landing Craft, Infantry) to be converted for rocket firing were fitted out in the States with ten automatic gravity-feed launchers, mounted outboard. At Guadalcanal, however, in preparation for the Marianas operations, LCI's were given a much greater rocket fire power: the ramps were removed and forty-two launchers were installed.

At first, it was believed that a landing craft with launchers should also perform its normal functions of troop or materiel transport. But to get the desired increase in rocket fire power, the craft often emerged as rocket gunboats. Not all rocket-firing landing craft underwent this metamorphosis, of course. Small support boats still functioned as wave directors, and larger

[10]It was planned that for the permanent detachments a spin-stabilized 4.5-inch rocket developed by the Army would replace the barrage rocket, and an Army launcher, the multibarrel "Honeycomb," which was mounted on a two-wheeled trailer, would replace the 12-round automatic.

support craft, with ten launchers in the well deck, were equipped to act as fire fighters after their rocket load had been expended. Thus mounted, barrage rockets performed at almost every Pacific landing operation after Arawe; at Biak, Wakde, Hollandia, Peleliu, Kwajalein, Saipan, Guam, and Tinian; at Leyte, Ormoc Bay, Lingayen Gulf; Iwo Jima, and Okinawa; 5568 rockets were launched at two beaches in the Leyte landings; 8000 on "D" Day at Iwo Jima.[11]

The Japanese contributed convincing evidence as to the effectiveness of the 4.5-inch barrage rockets. After experiencing a few barrage-rocket bombardments, they began moving their beach defenses back a thousand to twelve hundred yards, so that they would be out of range.

And this suggests the principal weakness of the barrage rocket—its range limitation. Offshore reefs might prevent the rocket-firing craft from coming close enough to reach the invasion beach with their fire. Or, even if there were no reefs, they were still limited in the extent of cover they could give from the water as the troops, having made their landing, advanced inland. To see defect or limitation is the starting point for providing a remedy. But how the Navy and the California Institute group did this belongs in a later chapter.

[11]By the time of the Iwo Jima operation another type of barrage rocket was in Service use, the 5-inch spin-stabilized round, of which 12,000 rounds were also fired on D-Day.

# CHAPTER XX

## *INTERLUDE*

THUS, by the end of 1942, the California Institute group had made major contributions in the Mousetrap and the beach barrage rocket. At the beginning of that year the group had consisted of about 100 people including research scientists, engineers, designers, and helpers. During the year, the work on Mousetrap and the beach barrage rocket brought new size and new problems. The most significant changes were in the setting up of several service sections to be added to the existing sections on design and test, on launchers and fire control, on photographic measurements, on propellant, on static tests, on special problems, and on report production.

### Developmental Engineering

The first crash production of 3000 rounds of BR's for Casablanca has been recounted. It was a successful job but it slowed down research and development, and used physicists as expediters and inspectors. Justified for the single emergency, it could be foreseen that the impact of emergency after emergency might end with no one available for further development. Under the circumstances a new section called Developmental Engineering was established in October 1942 with Trevor Gardner as head.[1] Space was rented for the new section in Pasadena a few blocks from the Institute campus. It was quite adequate for the four employees who then went to work for the production group. A year later these employees numbered 450; in December 1944, at the peak of the effort, they were 1144.

Quarters naturally bulged as well, and additional tracts were obtained on the outskirts of Pasadena. One tract had a building; on the other an inspection and assembly plant was erected with 38,000 square feet of floor area. In this plant, early in November 1944, the group celebrated the production of its millionth rocket.[2]

[1]At the time he joined the Institute group Gardner was Assistant to the Works Manager of the Plomb Tool Company of Los Angeles, where he was in charge of time-and-motion study. He had also given part-time courses at the Institute, under the Engineering Science and Management War Training Program, in time-and-motion study, improved methods of plant layout, and production control.

[2]Trevor Gardner was in general charge of the Developmental Engineering Section. His Assistant Supervisor, E. E. Tuttle, carried the responsibility of purchasing, priorities, and general supervision of the transportation division, besides contributing valuable legal advice. The detailed management of transportation was carried on by V. C. Jones, who had charge of over 150 vehicles. These supplied transportation not only for the produc-

Subsequent events justified the establishment of this section; not only did Navy requests for additional barrage rockets persist well through 1943 but subsequent rocket developments at the Institute were accompanied in almost every case by crash or interim production, saving months in getting the new rockets into Service use and correspondingly saving lives at the fronts. There were collateral advantages as well. Gardner's section provided ample supplies for research and field firing and for these the requirements were large during the development of aircraft and spin-stabilized rockets. Moreover the Developmental Engineering Section carried on procurement through a large number of subcontractors in the Los Angeles area; rocket design could thus be kept reasonably fluid and improvements introduced without interrupting production. Thus it was possible to avoid premature "freezing" of designs. Finally, the Developmental Engineering Section enabled procurement from Bureau of Ordnance prime contractors to be speeded up. These contractors could draw on the experience of the Developmental Engineering Section for the assembly and inspection techniques which had been worked out. Frequently they were supplied with gauges, jigs, and other equipment which the section had developed; and section representatives were available to serve as advisers to prime contractors when the latter were setting up their own production lines.[3]

The rapid expansion in rocket work is symbolized by the growth in personnel for the Developmental Engineering Section. This of course imposed loads on personnel procurement and accounting which could no longer be taken care of by the regular administration of the California Institute of Technology. As in many other large research developments personnel and accounting sections were set up to work exclusively for the rocket group. Verne E. Wilson was supervisor of the new Personnel Section;[4] the Accounting Section was headed by W. R. Stott until he resigned July 24, 1943, to become assistant comptroller of the California Institute; he was replaced successively by J. D. Heising and Hubert Ewart. Neither of these sections had a spectacular role to perform; each was essential to the group success.

tion activities, but for the whole Institute group. In meeting crash-production deadlines, much of the responsibility fell on Stanley Guelson, who was in charge of the organization and technical supervision of production of rocket metal parts. Production was speeded up also through the ingenious work of R. T. Stevens in designing production and inspection tooling.

[3]The section also prepared instruction pamphlets on inspection and assembly apparatus and techniques for the benefit of prime contractors.

[4]Trained as a psychologist, he had specialized in the practical applications of psychology to the problems of personnel selection and adjustment. At the time of his appointment, he was vocational consultant to the Pasadena city school system and consulting psychologist to the Los Angeles Bureau of Vocational Service. He had also taught courses in the use of psychological tests in business and industry in the ESMWT program conducted by the Institute.

Chart Y (pp. 138–139) presents the organization for contract OEMsr-418; a more extensive personnel list is furnished in Appendix 1c (p. 221).

## Ranges

It will be recalled that when the California Institute group began their operations in September 1941 they were given access to the Mojave Antiaircraft Artillery Range for test firing of rockets in free flight. This range was about 150 miles from Pasadena; for every test, rockets, launchers, controls, personnel had to be transferred over this distance; sometimes an unanticipated emergency called for a tool which had not been brought; the area was none too well suited for the work in any event.

The commanding officer of MAAR, Major (later Lieutenant Colonel) A. L. Friedenthal, had taken a personal interest in rocket development from its beginning on his post; perceiving that the first range location was unsatisfactory, he personally prospected the MAAR reservation which included a considerable area in the Mojave Desert and suggested Goldstone Lake as a better site for the rocket range.

The term "lake" as applied to a location in the Mojave Desert — and there are many places so designated — must not be taken too literally. Sometime in the remote past they were actually lakes; occasionally now, for a few days during the infrequent winter rains, they have a little water in them. But it soon disappears; and for the greatest part of the year they are dry lake beds, smooth-surfaced, flat, and hard-packed. Goldstone was such a lake — an almost ideal location for a rocket range.

The Goldstone Lake area was set aside for the use of the Institute group; a lease was negotiated with the owner of a decayed ranch, back about a mile from the lake, which was destined to become the Institute group's headquarters area. On February 4, 1942, a party of four arrived to make a preliminary survey and start work on range facilities. They found, in the ranch area, two buildings. One had formerly been used as a dwelling; it was lacking two doors and several windows, but could be made habitable. The other was a privy, as the report says, "in bad repair." During the following week, the house was cleaned and made moderately weather-tight. Miscellaneous equipment was installed in the house, and one room was converted into a small machine shop, with a lathe, a drill press, a bench grinder, a vise, and auxiliary tools. The south boundary of the range reservation was resurveyed. A new firing range was laid out, four miles of it extending across the lake bed, with range markers set up every 500 feet. Two launchers were emplaced for Chemical Warfare grenades (this was the rocket development to which the Institute group was giving top priority at this time); a target was built 600 feet from these launchers; and several camera positions were established.

# Chart Y

## ORGANIZATION FOR CONTRACT OEMsr-418 WITH CALIFORNIA INSTITUTE OF TECHNOLOGY

See Chart X on pages 46–47 for Divisional and OSRD organization

### Government Supervisors

Scientific Officer
F. L. Hovde
Chief Div. 3, NDRC
Asst. Sc. Officers
E. B. Bradford,
Tech. Aide, Div. 3

F. W. Cummings,
Tech. Aide, Div. 3
(Eng. and Trans. Office)
B. M. Norton,
Tech. Aide, Sec. L, Div. 3
Resident NDRC Officer, Cal. Tech.

### Contractor's Organization

Official Investigator
E. C. Watson
Asst. Comptroller Cal. Tech.
W. R. Stott

Director of Research
C. C. Lauritsen

Asst. Dir. of Research
W. A. Fowler

**Sec. I** Design and Development
W. A. Fowler Supervisor
- Projectile Group (1)*
- Interdepartmental Order Group (2)
- Fuze Group (3)
- Land/Amphibious Launcher Group (4)
- Special Launcher Group (5)
- Ballistics Research Group (6)
- Theoretical Research Group (7)
- Drafting Staff (8)
- Mechanical Staff (9)

**Sec. II** Aircraft and Ballistics (10)
C. D. Anderson Supervisor
- Fire Control Group (11)

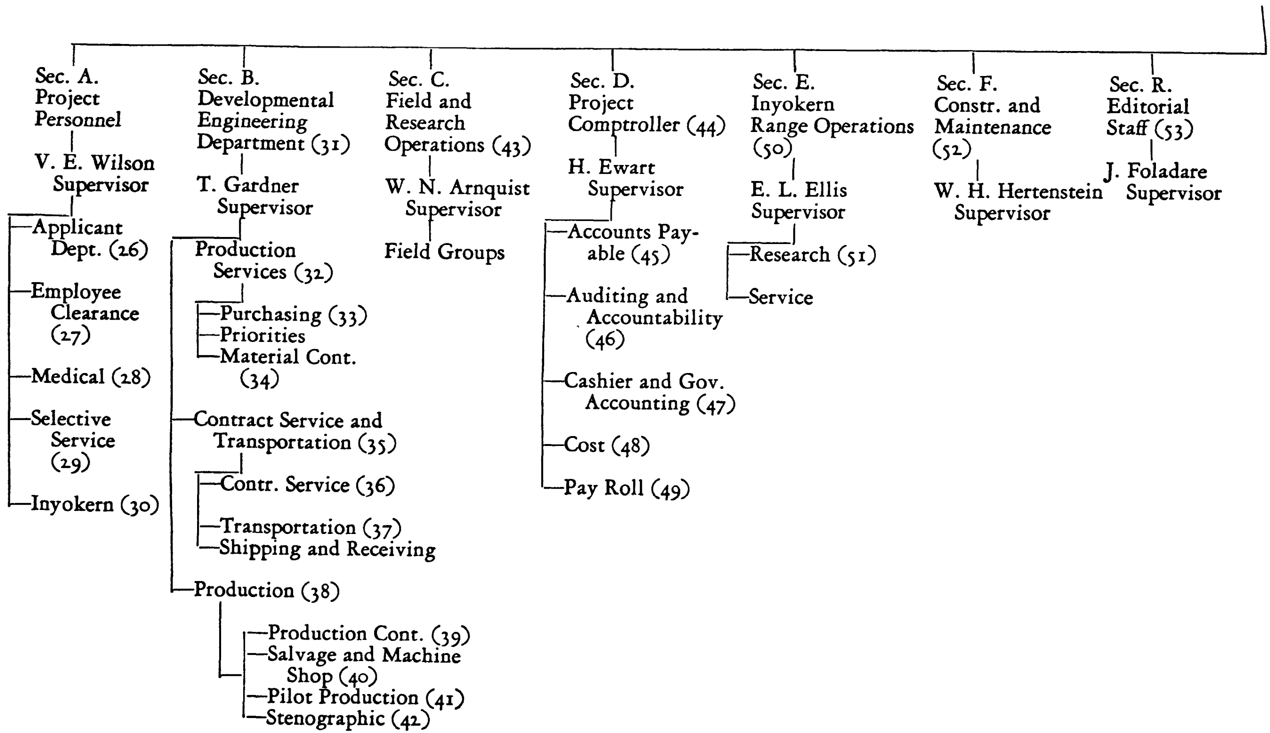
**Sec. III** Photographic Measurements and External Ballistics (12)
I. Bowen Supervisor

**Sec. IV** Underwater Properties of Projectiles (13)
Max Mason
L. B. Slichter
Supervisors

**Sec. V** Propellant and Interior Ballistics
B. H. Sage
D. S. Clark
W. N. Lacey
Supervisors
- Res. and Dev. (14)
- Tech. Control (15)
- Operations (16)
- Experimental Prod. (17) Control and Shipping
- Inspection (18)
- Static Firing (19)
- Magazine (20)
- Service (21) Operations
- Inyokern Operations (22)

**Sec. VII** Torpedo Launching
F. C. Lindvall Supervisor
- Research (23)
- Engineering (24)

**Sec. VIII** Special Ballistics (25)
W. R. Smythe Supervisor




* Scientist and supervisory personnel are listed in Appendix 1c according to the numbered groups. Total number of all employees at maximum in 1944 exceeded 3000.

The lists as prepared show personnel at the end of 1944.

For the immediate future, the improvements planned were a concrete launcher emplacement at the edge of the lake, the establishment of permanent camera positions, the installation of permanent control wiring, the setting up of sleeping quarters for the range crew, and the installation of an incinerator and a water tank.

Thus the Goldstone range started. It continued to be enlarged and improved until nearly the end of the war. Space is too limited to follow the development in detail; only the essential features of the facility can be outlined.

In the headquarters area, office space and a plotting room were laid out, first in the old ranch house, later in a new building. Permanent and comfortable housing accommodations were built; a mess hall and recreation room were provided. A water-supply and -distribution system was put in, a power plant installed, shop facilities enlarged.

On the range, a concrete launcher emplacement was built at the edge of the lake bed. In addition to the early range markers, spotting towers were erected, one directly behind the launcher emplacement and others down range. The positions of these spotting towers were carefully surveyed with reference to the centerline of the range. Spotters in the towers, with an instrument known as a spotting rake, got the bearings of the impact points of the rockets as they were fired. These data were plotted on sheets which were scaled to the range and had the spotting positions added. Hence, the intersection of two bearings from spotting stations would locate the impact point of each rocket, and the range and dispersion of each round fired could be easily and quickly determined. Ovens and cooling chambers were built, similar to those installed in Eaton Canyon, described later. Camera stations were also set up, both for standard motion-picture cameras and for an acceleration camera, all of these being actuated from the firing station.

In the planning and supervision of the construction at Goldstone, the chief burden fell on the Institute's Superintendent of Buildings and Grounds; Wesley Hertenstein,[5] who carried this responsibility in addition to similar work in connection with Eaton Canyon. Through most of the Goldstone activity, C. F. Robinson[6] served as range supervisor. When he returned to Pasadena in the summer of 1944 to work with the research group in Pasadena, he was succeeded by R. H. Cox,[7] who had for some time previously been assistant supervisor. The Goldstone range continued in operation for a few weeks after the end of the war, in order to wind up a research program. Then the physical equipment which was still serviceable was salvaged and the range was closed down.

[5]An Institute graduate in civil engineering.

[6]A graduate student in physics at the Institute, he interrupted his graduate work to join the rocket group in the fall of 1941.

[7]An Institute graduate in civil engineering.

Thanks to action by the Navy, the California Institute group had access to another important and near-by range facility. This was at the United States Marine Corps' Camp Pendleton, which included a large tract of land extending along the southern California coast between San Clemente and Oceanside. Through an appropriate chain of official actions, commencing with a recommendation from the Chief of the Bureau of Ordnance of the Navy, the Commandant of the United States Marine Corps arranged for one of his West Coast camps to be host to this activity, pursuant to that part of the specification which said the West Coast range should be within a reasonable distance of the California Institute of Technology. NDRC in turn authorized the Institute group to co-operate in the establishment of a range on Marine Corps property, and Fowler and others helped to survey the terrain.

The Institute group's part in setting up the facility consisted in locating the ranges and co-operating in the design and supervision of construction and in supplying equipment. After the ranges were in operation, partly for training programs, the Institute group, working closely with the rocket battalion, had the use of the facility for range firing in connection with rocket and launcher development. A resident staff was maintained at Pendleton, headed first by J. E. Thomas, succeeded in turn by Earl Skene and N. L. Prindiville.

Both land and sea ranges were provided at Camp Pendleton. The land range was located a few miles back from the ocean. The general pattern of Goldstone was repeated — permanent launcher emplacement, spotting stations, ovens and cooling chambers, plotting room. Magazines for the storage of motors, heads, and fuzes were constructed at a safe distance back in the hills. The sea range was located near the north boundary of the camp, with the launcher emplacement and spotting stations constructed on the bluff for firing seaward.

In the headquarters area, housing and auxiliary structures were erected for a rocket battalion which, under the command of Major (later Lieutenant Colonel) Valentine Hoffman, carried on a testing and training program for the next year.

The additional range facilities provided at Camp Pendleton came at an opportune time. The work of the Institute group was steadily expanding, with a corresponding increase in range firing. In particular, the summer of 1943 saw the beginning of the development of forward-firing aircraft rockets, which will be described in the next chapter. Aircraft firing was carried on at Goldstone, with a consequent tax on the time and facilities of that range. Pendleton could take care of the overflow, particularly the launcher developments for the 4.5-inch barrage rocket and the experiments with rockets as weapons for Marine paratroopers. Pendleton also had the advantage that rockets with high-explosive heads could be fired there,

whereas at Goldstone the practice was to fire only rockets with heads inert-loaded to the standard weight of Service rounds. Hence, data could be secured at Pendleton on fuze functioning and on such matters as the effective area of barrage-rocket bursts, and, with later rockets, their penetrating power.

The summer of 1943 saw plans made for still more range facilities. The Bureau of Ordnance saw the need for a permanent, large-scale ordnance test station. In the long-range view, this station should be capable of testing many types of Navy ordnance. Since rocket development, particularly aircraft rockets, had a high priority, it was decided that at the start the emphasis should be put on rocket work, and the station should have ranges and facilities for all types of rocket firing. After considering several different locations, the Bureau finally chose a large area in the northern part of the Mojave Desert near the town of Inyokern, California. Several considerations influenced this choice. In the first place, the winter was mild enough so that there would be only a very small seasonal interference with range work. Since most of the area to be included in the station was public land, there would be only a small problem of buying out private owners. And finally, Inyokern was within a reasonable distance, 160 miles, of the California Institute, where rocket development for the Navy centered. The development of the Naval Ordnance Test Station, Inyokern, and its connections with the Institute group will be outlined in a subsequent chapter.

In the fall of 1944, the Army began a program of developing launcher installations for a number of planes and a project was set up through NDRC for launcher developments and training. Cal. Tech. personnel from the Range Operations and Aircraft Launcher and Ballistics Sections maintained a station at Muroc, California, while laying out an aircraft rocket range, and afterwards supplied information on launcher installations, range operation, and instrumentation.

## Apparatus

It is easy to forget that any development as large as the rocket program necessarily entails the collateral development of much new apparatus and many new techniques; usually, indeed, these have to come first. Often they remain to be more important than the pragmatic piece which has made the spectacular headline.

This is a routine observation for the person familiar with the processes of scientific research and development in which there are no miracles. But it is worth laboring a little so that it may be remembered.

The work of the California Institute group was no exception in this particular; it is worth while to continue to interrupt the narrative briefly to point out a few of these achievements; those selected are epitomes of others and have interest in themselves. One group centers around static test firing

at the Eaton Canyon facility, the other around the section on photographic measurements.

## Static Test Firing

By the summer of 1943, there had been a great expansion of equipment and operations at Eaton Canyon. Not only had additional extrusion presses gone into operation, but related facilities had expanded correspondingly — facilities for preparing sheet ballistite for the presses, for finishing the extruded grains, for making igniters, for motor loading, and for static test firing.

In connection with the last, three pieces of equipment are of particular interest. The first was the ovens and cooling rooms. In developing a propellant grain, one of the essential things to know is how it will perform at various temperatures, since if it is to be acceptable for Service use, it must perform reliably through a wide temperature range. With cooling rooms and ovens, grains for static firing can be given any temperature between —50° F. and 160° F. Firing at various temperatures is necessary not only in the development of new grains; in production of grains for range firing and for Service use, a certain number from each lot must be fired at a diversity of temperatures to make certain that an acceptable standard of performance is being maintained. The cooling rooms and ovens also proved useful in finding out for the Navy whether there is any significant deterioration in rocket propellant from prolonged storage at comparatively high temperatures, or from rapid alternations of temperature extremes.

The second piece of special equipment was a partial-burning apparatus. In developing a propellant grain, it is useful to know, among other things, what is happening to the grain during the course of its burning — to what extent, for instance, and in what places it is being eroded by the flow of gas toward the nozzle end of the motor tube, or whether the burning is taking place evenly. The partial-burning apparatus was devised by John McMorris as a means of answering such questions. It consists, in essentials, of an arrangement for holding a static-firing motor vertically over a water-filled pit, and a device which can be set to eject the grain into the water at any predetermined instant during its burning life. The sudden drop in pressure as the grain is ejected stops the burning and the water prevents reignition. Thus the partially burned grain is available for inspection and analysis.

The third type of equipment — and the most ingenious so far as design was concerned — was a pressure-time-recording apparatus. In rocket motor design (as well as sample testing of production lots of propellant grains), it is necessary to know the pressures developed during burning of the grains. Measurement of the maximum pressure developed during burning was not too difficult; what was needed, however, was an apparatus which would give a continuous pressure record during the whole burning life of the grain.

To secure such a record, the Institute group developed a Bourdon gauge optical system.[8]

This system functions as follows. Into the static-firing motor tube, which is secured to a concrete block in the firing bay, two copper tubes are tapped, one near the front of the motor, one near the nozzle end. These tubes lead through the concrete wall of the firing bay into the recording room. There each terminates in a Bourdon gauge, which is in essential simply a curved piece of elliptical tubing closed at the end. (The common Bourdon gauge is a half circle or more. For this application, it was found more satisfactory to reduce the gauges to quadrants.) The copper-tubing line is filled with oil. When the motor is fired, the internal pressure is communicated via the oil lines to the Bourdon gauges. With increased pressure the latter tend to straighten out, the amount of deformation being proportionate to the pressure.

The next problem is to translate this deformation movement into accurate pressure records. As the group worked on the development of the apparatus, various mechanical linkages were tried. The most satisfactory solution, however, consisted in mounting small mirrors on the free ends of the gauges. A light source is activated when the firing key is pressed; a light beam is reflected from each mirror onto sensitized paper held on a revolving drum.

Thus, as the grain burns, the light beam traces a continuous line on the sensitized paper. Since the latter is scaled horizontally for time and vertically for pressure, a continuous curve plotting pressure against time is available as soon as the paper is developed — that is, within one minute after the static firing is over.

The Bourdon gauge optical system, like most apparatus, underwent many improvements and refinements in the course of its development. It proved to be so useful an instrument in rocket work that the Institute group was asked to make a number of these Bourdon gauge optical units for Army and Navy facilities where static firing of rocket motors is regularly carried on.

## Photography

The original organization of the Institute group in the fall of 1941 included a section on photographic measurements and exterior ballistics, headed by I. S. Bowen. The duties of this section were twofold: to provide a general photographic record of the activities of the Institute group, and to secure accurate data on rocket velocities and accelerations, for the design sections. Standard motion-picture and still cameras were used for the first.

[8]Some of the most useful suggestions in developing this apparatus were made by Thomas Lauritsen, who headed the projectile group of the Projectile Design and Field-Testing Section.

For the second, Bowen designed a precision high-speed ribbon-frame camera. The first model had a speed of 166 frames per second. Its field of view along the trajectory was about one half the camera-trajectory distance; normal to the trajectory, about one fiftieth of the camera-trajectory distance. Its focal range was 3.77 feet to infinity; its focal length, *f.* 2.7. This camera, which became generally known as the C.I.T. Acceleration Camera, went through a number of refinements and improvements. It became standard equipment, not only on the Institute range, but on others where precision records were desired. During the whole course of the research and development work of the Institute group, a subsection under Bowen's direction was kept busy on the measurement of the records made with the acceleration and other high-precision cameras, and the reduction of these measurements to fundamental data. This work was not only essential to the design and development groups, but it provided the basis for a comprehensive monograph on the exterior ballistics of rockets which, written in the fall of 1945, constitutes one of the Institute group's permanent contributions to the art of rocketry.

About the middle of 1942, the Office of Strategic Services made available for the Institute group a photographic unit headed by Lieutenant Commander (later Commander) A. L. Gilks, U.S.N.R. This removed from Bowen's section most of the work of photography for general record purposes. Commander Gilks was a cameraman of great skill and wide experience. He and his crew were on call for all the test and demonstration firings of the Institute group; their films provided a full and effective record of the group's activities and were particularly useful in keeping staff officers in Washington and elsewhere informed of developments in the Division 3 rocket-ordnance program. In addition, they made for the Navy a number of indoctrination and training films.

## Liaison

As the rocket program burgeoned it became increasingly necessary for the California Institute group to have direct liaison with Army and Navy, especially the latter. Each new development brought with it a special liaison group and of course officers were sent from time to time from Washington; many visits back and forth were paid by division and contractor personnel. Nonetheless the liaison problem had become of such magnitude that it was desirable for the Navy to have a resident liaison officer.

To this post Rear Admiral Ralston S. Holmes, U.S.N. (Ret.), was assigned by the Navy immediately after his retirement at the end of 1942 as Commandant of the Eleventh Naval District. On subsequent dates other officers were added to his staff; Captain James L. King, U.S.N. (Ret.), in 1943 when he ceased to serve the torpedo section of the Bureau of Ord-

nance; Commander C. E. Haugen, U.S.N. (Ret.), later in the fall of the same year.[9] Lieutenant J. D. De Santo, U.S.N.R., also had duty in Admiral Holmes's office for a time in 1943, but then became proof officer, first at Camp Pendleton and later at Inyokern. Lieutenant T. A. Barnum, U.S.N.R., was stationed at the Institute in the latter part of 1943 as Resident Inspector of Naval Materiel and continued there in that capacity until the fall of 1945.

Although by far the greater part of the work done by the Institute group was for, and in co-operation with, the Navy, the Army also stationed liaison officers at the Institute. So far as rocket developments were concerned, in the main they held, as it were, a watching brief; they had other duties, however, in connection with other research and development activities which the Institute was conducting under separate contracts with NDRC or directly with the Army.[10] Colonel R. S. Parrott and Captain (later Major) R. B. Staver of the Ordnance Department were the first liaison officers assigned to the Institute. To them was added Colonel W. H. Joiner, AAF, in the summer of 1943. Colonel Parrott was retired in the fall of 1943, leaving Colonel Joiner and Major Staver as the Army representatives until the fall of 1944, when the group was augmented by Colonel L. A. Skinner, OD, (who had been associated earlier with Section H in the East), Major W. M. Black, Captain G. Svehla, and Lieutenant R. C. Clark, AAF.

[9]Admiral Holmes, Captain King, and Commander Haugen continued their work in liaison until the office was closed on October 10, 1945.

[10]Like the Army liaison officers, Admiral Holmes and his assistants were also concerned with other contracts between the Institute and the Navy or other divisions of NDRC.

# CHAPTER XXI

# *FORWARD-FIRING AIRCRAFT ROCKETS*

IF 1942 for the California Institute group was the Mousetrap and barrage-rocket year, 1943 was pre-eminently the year of forward-firing aircraft rockets. It will be recalled that there had been considerable interest in a plane-to-plane rocket early in the history of Division 3, NDRC, and that one of the earliest applications considered for the 4.5-inch rocket developed by Section H was for aircraft firing.

With the California Institute group, however, the original motive for the development of its first forward-firing aircraft rocket was the need for additional antisubmarine weapons, and the lead which the Institute rocketeers followed in that development came, as in the case of some of the earlier rockets, from England.

During 1942, the British, in order to provide additional armament for aircraft on antisubmarine search and patrol, modified their 3-inch fin-stabilized rocket (the UP-3) by substituting a solid steel shot for the conventional head. For aircraft firing, pairs of small-diameter rods were attached to the under side of the wings (four pairs to a wing). Lug bands with curved ends suspended the rockets from the rods. The rockets were loaded into the launchers from the front, by sliding these lug bands back along the pair of rods. A retaining device then held the rockets in place until the propellant was ignited, when the retainer was freed and the rocket was guided in its initial flight by the lug bands sliding along the rods.

Land-based medium and long-range bombers assigned to antisubmarine warfare began using this new weapon late in 1942 or early in 1943, and by June 3, 1943 they had chalked up a score of nine verified submarine kills.

## THE 3.5-INCH AIRCRAFT ROCKET

British success against submarines with forward-firing aircraft rockets aroused great interest on this side of the Atlantic. British rockets were secured for testing, and the possibility of adopting them for American use was seriously considered. Since Fleet Air West Coast and the California Institute had already teamed up for the development of the antisubmarine rocket for retro firing from aircraft, they were the logical combination to undertake the testing of the British rocket and, possibly the development of an American counterpart. As in the past, the California Institute sup-

plied the technical rocket know-how, and ComFair West Coast supplied the planes and pilots.

On July 12, the first British rockets were ground-fired on the Goldstone range, where much of the retro firing had been carried on; the first air firing was done on July 14.

The results were generally satisfactory, but in the meantime it had been decided that an American rocket of the same general type as the British should be developed. There were several reasons for this decision. Depending on a supply of rockets from Great Britain introduced uncertainties: production might not be sufficient, and transportation problems might be difficult. To duplicate the British rockets here would have necessitated new facilities for making the British propellant, cordite (which was not manufactured in the United States), and learning another dry-extrusion technique.

Hence, on June 7, 1943 the Commander in Chief, United States Fleet, directed the Bureau of Ordnance and the Bureau of Aeronautics to collaborate on a project for the development of aircraft rockets and launchers to be set up at the California Institute with Commander Fleet Air West Coast assisting.

Fortunately, by this time the California Institute group had accumulated considerable experience with the design of high-performance motors of the type that would be needed for a forward-fired aircraft rocket.

One of their earliest projects, it will be recalled, was the development of a high-altitude antiaircraft rocket. Work on this had proceeded somewhat slowly because the thick-web propellant grain which it required was not available until the 8-inch extrusion press went into operation in April 1942. By that time, however, the urgency of the Mousetrap program (and, a little later, the barrage-rocket development) relegated the high-altitude antiaircraft rocket to relatively low priority. But some work was carried on and experience accumulated. Then, in the spring of 1943, as a result of informal discussions with representatives of the Navy Department, work was begun on a 3.25-inch motor with a single propellant grain of solventless-extruded three-ridge ballistite. By March 1943 tests had begun on this motor, and by the end of April the maximum weight of tubular grain for the motor had been extensively tested, both statically and in the field. This amounted to only 6.2 pounds of tubular propellant with a 2.5-inch outside diameter by 0.38-inch inside diameter. Grains weighing as much as 6.8 pounds had satisfactory results in static firing, but failed under the high accelerations of field firing. The tubular grain therefore was inadequate.

In April, therefore, the propellant section of the Institute group decided to make extrusion dies for a propellant grain of cruciform cross section similar to that with which the British were getting good results. A cruciform grain would permit a higher density of powder loading in the motor

tube and still satisfy the limiting necessities of sufficient free-flow area for the gases generated during burning. It will be remembered, however, that one of the principal desiderata of a propellant grain is a constant surface area during burning. A cruciform grain does not satisfy this condition; its area decreases during burning. Hence, it was necessary to apply plastic inhibiting strips to part of the grain in order to secure a constant surface area during burning. So, in addition to the problem of extruding cruciform grains, there was the further problem of determining the extent and optimum pattern of the inhibiting strips. Nine-pound cruciform grains were available for static tests by late April; the first range tests were conducted on May 15, 1943.

On that same date (May 15), this exploratory program was formalized by a request from the Chief of the Bureau of Ordnance, through the Navy's Co-ordinator of Research and Development, to NDRC for the development of a long-range rocket (at least 10,000 yards) for firing from shipboard against shore targets. The request specified that the motor be 3.25 inches in diameter, and the propellant, solventless-extruded ballistite. A nose fuze designed to function instantaneously on land or water impact was also specified. While the weight of the head was not set down as a requirement, initial experimentation was directed toward a head weight equal to that of the 75-mm. shell, i.e., 13 pounds. The development of suitable launchers was also requested.

Since the motor development was already well under way when this request was made, work on the long-range ship-to-shore rocket could go ahead rapidly. This motor fitted with a 13-pound smoke head was soon undergoing range tests. When fitted with a low-drag nose fuze, this rocket attained a range of approximately 10,000 yards; and consideration of its aerodynamic characteristics indicated that very nearly the same range could be maintained if the head weight was increased to 20 pounds.

The performance characteristics of this rocket were very close to those of the forward-firing aircraft rocket already developed by the British. Hence, the Institute group could go ahead rapidly on the development of an analogue of the British rocket when the official decision was made to embark on a program of development of aircraft rockets. Formal action was instituted on July 7, 1943 by the Commander in Chief, United States Fleet. NDRC had formally assigned such a project to the California Institute group on June 19, 1943. To facilitate the work, ComFair West Coast set up in July an experimental rocket squadron, which supplied planes and pilots for all the experimental firing. And under authorization from the Bureau of Aeronautics, ComFair West Coast also collaborated on launcher development.

Work had already started before the formal assignment. On June 18 rocket motors had been fired, for testing blast effect, from a TBF (Grum-

man Avenger) wing section. These tests showed that no significant blast damage resulted from rockets fired from launching rails mounted on the under side of the wing.

During the next six weeks work went ahead rapidly on improvement and standardization of the motor and on launcher installations for TBF and PBY aircraft. Pending the approval of the Institute rocket for air firing by the Applications Safety Committee, extensive aircraft-firing tests were made with British rockets. Early in August, Commander Fleet Air West Coast established a naval auxiliary air station at Sandy Beach on Salton Sea, for the purpose of determining the underwater trajectories of aircraft rockets. Since the rocket being developed by the California Institute was designed primarily as an antisubmarine weapon (it could also be used against light shipping), it was important to know its underwater behavior. The Salton Sea range was used extensively for this purpose and also for training combat pilots.

The first air firing of the Institute rocket took place late in August, with Lieutenant Commander (later Commander) Thomas F. Pollock piloting the plane. Even before this date, however, various official actions by the Bureau of Ordnance showed the Navy's interest in getting this new weapon into Service use as rapidly as possible. On August 7 the Chief of the Bureau of Ordnance requested production at the rate of 10,000 rounds per month for four to six months, in order to provide ammunition for extensive Service tests prior to the estimated date of Navy production. This production job was assigned to the Institute by NDRC on August 28. On August 10, the Institute group was assigned the further task of providing 200 launcher installations for TBF aircraft. These were the first type of aircraft to be so equipped because they were used from escort carriers on convoy duty. The object in having the Institute provide the 200 initial installations was primarily to furnish enough experimental material to permit stabilization of the launcher design for Navy production. By the middle of August, the Bureau of Ordnance had assigned to the development and production of aircraft rockets the highest priority among antisubmarine weapons.

This forward-firing aircraft rocket (3.5-inch AR) was the first of a series of aircraft rockets developed at the California Institute. It has a 20-pound solid steel shot head 3.5 inches in diameter, and a 3.25-inch motor carrying a cruciform ballistite grain weighing about 8.6 pounds. The over-all length is about 55 inches; the total weight, about 55 pounds. The velocity is about 1175 feet per second. (To this, of course, must be added the velocity of the aircraft at the moment of firing.) The earlier models of this rocket had a lethal range under water of about 50 feet, when fired from a plane in a dive angle of 15°. That is, the solid shot head would rupture the pressure hull of a submarine after underwater travel of that distance.

This head had a hemispherical nose of the same diameter (3.5 in.) as the body of the shot. An improved head, with a sphere-ogive nose, that is, a long taper to a small hemispherical nose, improved the underwater trajectory, extending the lethal range to about 130 feet.

This rocket, like all fin-stabilized aircraft rockets, has a much smaller dispersion than ground-fired rockets. The reason is that, at the moment of firing, the aircraft rocket already has the velocity of the plane which is carrying it, and hence the fins exert a much greater stabilizing effect immediately. The 3.5-inch aircraft rocket, for instance, has a mean dispersion of only 4 mils.

## Launcher Developments[1]

The first launcher which the group developed for aircraft rockets was considerably simpler than the one used by the British. It consisted of a slotted rail made of dural. Lug bands with buttons were fitted to the rockets. These buttons, in loading, slid along the slot, holding the rocket in place, and then, after firing, guided it along the rail in its initial flight.[2] The standard installation was four rails under each wing, giving the plane a total armament of eight rockets. The electrical firing system was arranged so that the rockets could be fired in singles, in pairs, or in salvo. In submarine attacks, the general practice was to fire pairs.

The first sets of rails were 90 inches long, projecting beyond the leading edges of the wings. It is obvious that any such set of excrescences would be the despair of an aerodynamicist, who wants above all to keep the exterior surfaces of his plane "cleaned up." The eight sets of launcher rails do in fact set up drag enough to reduce the air speed of the TBF about 17 knots. This reduction in air speed was acceptable, perhaps (though pilots didn't like it), for aircraft engaged primarily in submarine search and attack, but if fighter planes were to be equipped for rocket firing, no such reduction in speed and consequently in range and maneuverability could be tolerated. The long rails also complicated the problem of loading rounds with the wings folded on a carrier.

A partial remedy was found in "bobbing" the rails to 70 inches. When it was found that reduction in launcher length did not materially affect

[1]Launcher development was the responsibility of the Launcher Section of the Institute group. With the growing importance of the aircraft-rocket development, a subsection on aircraft launchers was set up in July 1943, with C. D. Anderson as supervisor. This subsection continued its special work until September 1945. Anderson had been one of the supervisors of the general Launcher Section for some time prior to July 1943; he got his first initiation into aircraft-launcher problems when the retro-bomb development began over a year earlier. The aircraft-launcher subsection also took over problems of the ballistics of aircraft rockets, accumulation of data for firing tables, etc.

[2]This was the same type of launcher that was used by the Russians when they armed their Stornovik planes with rockets for antitank warfare. They were the first to use forward-firing aircraft rockets.

accuracy of fire, permission was issued to cut off the forward part of the rails. The more satisfactory solution, however, was provided by the development of the zero-length launcher. This launcher consisted merely of two posts attached to the under side of the wing. The rocket was suspended in slotted plates on the bottoms of these posts. The slots provided only about a half-inch of launcher travel when the rocket was fired, but tests showed that there was no significant loss of aiming accuracy with the zero-length launchers, especially from higher-speed planes and, with the launching posts streamlined, they permitted a great reduction in drag. Even the fastest fighter planes had their top speed reduced by only 6 to 9 miles an hour by the new launcher.

The group began investigating the possibilities of zero-length launchers soon after they began work on the rail-type launcher. The rail launchers, however, were the first to be standardized and to go into Service use, in the fall of 1943. The zero-length launcher eventually supplanted the rails, but did not go into Service use until late spring of 1944, and zero-length replacements for rail launchers were not available until the following summer. By October, the zero-length had been selected as the standard type for all carrier-based aircraft. As rapidly as possible, zero-length launchers were incorporated into plane production.[3]

## Production of Rounds for Service Use

The California Institute began regular deliveries of 3.5-inch aircraft rockets to the Navy in September 1943. In October, by agreement with the Bureau of Ordnance a number of simplifications and improvements were incorporated in production rounds. Early in September, the question was raised of increasing the total production; it was then estimated that 100,000 rounds would be needed before Navy production was well underway. Thanks to the fact that the Institute group had set up the special production section nearly a year earlier, they were in a much better position to undertake such a job. A canvas of available facilities indicated that metal-parts production could be built up to a rate of about 1000 per day. The Propellant Section in Eaton Canyon could extrude grains and load motors at the rate of 450 per day. The Navy agreed to set up facilities to take care of loading beyond 450 units per day.

Because such a production program would impose considerable strain on the Institute's facilities, it was further agreed that the Navy should attempt to get British rockets from England, and if successful, cancel a corresponding part of the Institute's interim production. The great urgency

[3]Toward the end of the war the Navy developed a launcher which might have supplanted the zero-length. This consisted of a single pylon, from which the rocket was suspended at its center of gravity. Its great advantage lay in a still further reduction of drag.

of the situation and the uncertainty as to how many British rockets could be obtained made it necessary for the Institute to plan for the full 100,000 rounds. It is fortunate that this was done, for the number of British rockets received was not large enough to justify decreasing the commitment, and the project was carried through to completion in March 1944, overlapping by a few weeks the first Navy production.

## The 5-Inch Aircraft Rocket

In rocketry as in most other human activities, one thing leads to another. The 3.5-inch rocket served admirably for antisubmarine warfare. But its accuracy suggested that aircraft rockets could be used advantageously against other kinds of targets, where a high-explosive head would be needed for satisfactory results. A variant of the original 3.5-inch aircraft rocket was developed with a TNT-filled head, but was not satisfactory because of the small amount of high explosive which it carried. The next step was to provide a larger head.

The result was the 5-inch aircraft rocket (5-inch AR). For the head, the California Institute used an adaptation of the standard 5-inch antiaircraft common shell, which weighs about 45 pounds and carries 8 pounds of TNT. The motor was the 3.25-inch already developed for the 3.5-inch AR. The first models were provided with an instantaneous nose fuze armed by the rotation of a small propeller during the first part of the rocket's flight. For targets where some penetration was desired before detonation of the high explosive, however, the instantaneous nose fuze was unsatisfactory. Hence, later models were provided with a delay base fuze, armed by the action of a diaphragm operated by a buildup of pressure from the motor. When the base fuze became available for Service rounds, the general practice was to equip each round with both nose and base fuzes. If penetration was desired, the pilot could fire the rocket with the nose fuze "safe" (that is, prevented from arming).

The 5-inch aircraft rocket presented no particular problems. The motor development had already been taken care of; and the launchers, both rail and zero-length, were as suitable for the 5-inch as for the 3.5-inch. First firings of the 5-inch rocket were made in September 1943; rounds were issued for Service use by the end of the year. This first model was intended chiefly for fragmentation and anti-personnel effect. Other models were later developed, notably one with a thicker-walled (semi-armor-piercing) head for greater penetrating power.

The 5-inch aircraft rocket has an over-all length of about 65 inches and a total weight of about 80 pounds. In rocket design, as in most other activities, you can't get something for nothing. In the development of the 5-inch aircraft rocket, the something that was got was a significant amount of high explosive and steel in the head. But the price that had to be paid

was an increase in weight, and that, with the same motor and the same amount of propellant, meant a loss in velocity. The 3.5-inch rocket had a velocity of about 1175 feet per second; the increased weight of the 5-inch AR reduced its velocity to about 710 feet per second.

## Holy Moses

High velocities for aircraft rockets are desirable on many grounds. High velocities mean flatter trajectories and greater kinetic energy and hence greater penetrating power. At the time the development was undertaken, pilots, especially of carrier-based aircraft in the Pacific, were asking for something with more hitting power than the 5-inch aircraft rocket, to fire against Japanese shipping and land targets. Higher velocities also permit accurate firing at longer ranges, a fact of the first importance if the targets are defended by antiaircraft fire.

Hence, the next obvious step was to develop a rocket with the velocity of the 3.5-inch AR and the high-explosive load of the 5-inch. Such a development would, of course, necessitate the design of a new motor. The Propellant Section of the group began exploring the problems involved in a propellant grain large enough for such a rocket. They soon had the answer: a grain of solventless-extruded ballistite with a cruciform cross section, a web thickness of 1.55 inches, an outside diameter of 4.19 inches, a length of about 40 inches, and a total weight of 24 pounds. By early December, motors using this grain were being tested in static firing and by the middle of the month the first rounds had been ground-fired on the Goldstone range.

This rocket was officially designated the 5-inch high-velocity aircraft rocket (5-inch HVAR or, more simply, the HVAR).

One interesting and useful innovation in design was introduced in this motor. For the 5-inch HVAR, instead of the customary single central nozzle, the nozzle plate had eight peripheral nozzles; then, in addition, there was a central nozzle with a blowout disk which held until the motor pressure reached 2400 pounds per square inch (corresponding to a temperature of 125° F.). At this pressure the closure disk blew out, bringing the central nozzle into action and thus increasing the effective nozzle throat area. This gave the rocket a safe operating range of 20° F. to 125° F.

After the motor design was fairly definitely established, the development followed the familiar routine of static firings to accumulate data on the internal ballistics of the complete round. Blast testing for the effects of firing the rounds from launchers on planes was carried out with particular thoroughness, because this was by far the most powerful aircraft rocket motor yet produced by the California Institute group. Finally came air firings, with the conclusion that the rocket justified the hopes of its de-

signers and would constitute a very powerful addition to aircraft armament.

The official name of the rocket was the 5-inch HVAR. But during its development it was nicknamed Holy Moses. This nickname, which was applied by Conway Snyder of the projectile design group, passed into general use as an appropriate tribute to the power of the rocket.

Some of the characteristics of Holy Moses have already been mentioned. The others are worth listing: Over-all length, 72 inches; total weight, 140 pounds. The velocity (at 70° F.) is 1375 feet per second; so the designers got better than the 1175 feet per second of the 3.5-inch AR. Eventually two models were produced; one with a base fuze and a semi-armor-piercing head; the other, with both base and nose fuzes. Since enemy shipping would be among the probable targets for Holy Moses in Service use, a sphere-ogive head was developed (following the general pattern of the sphere-ogive head for the 3.5-inch AR), to give the round a longer, flatter underwater trajectory and hence a better chance of holing the hull.

Launchers were no problem. Both rail and zero-length launchers were as suitable for Holy Moses as for the previous aircraft rockets. By the time Holy Moses went into Service use, however, the zero-length launchers, because of their lower drag, were superseding the rails, and the types of aircraft which would be armed with HVAR were equipped with zero launchers.

By the spring of 1944, the California Institute was involved in interim production of Holy Moses. The Production Section was turning out 150 to 200 rounds daily, and had undertaken a commitment to produce 35,000 rounds by October 1944. Ultimately, when Bureau of Ordnance production reached sufficient volume Holy Moses would probably have almost completely replaced the 5-inch aircraft rocket as a Service round; but as late as March 1945 production was still limited, and distribution of rounds was under allocation by the Joint Chiefs of Staff since the AAF had put in bids for them.

Holy Moses first went into combat use in July 1944, under dramatic circumstances which will be detailed later.

## Subcaliber Aircraft Rockets

The interval between the experimental development of the first aircraft rockets by the California Institute group and the introduction of those rockets into Service use was a relatively short one. The result was that the rockets were being fired against the enemy and larger and larger requirements for them were developing before the Bureau of Ordnance could secure any considerable volume of production from its contractors. And as aircraft rockets demonstrated their usefulness, the Navy kept expanding its program for training pilots to use them. Adequate training, of course,

could not be done without the firing of rockets. All this combined to create an acute shortage of ammunition.

Anticipating some such difficulty, and thinking also in general terms of the advantages of economizing on propellant, the Institute group (with Bureau of Ordnance co-operation) began, quite early in the aircraft-rocket development program, to design a subcaliber round which could be used, instead of the full-size ammunition, for pilot training.

These subcaliber aircraft rockets (commonly known as SCAR's) were designed with a 2.25-inch motor carrying a 3-ridge tubular propellant grain of solventless-extruded ballistite, similar to the grain already developed for the 4.5-inch barrage rocket. In general external form and aerodynamical characteristics, the SCAR's matched the full-size rockets. By varying the amount of propellant, one SCAR had a trajectory approximating that of the 3.5-inch AR and the 5-inch HVAR (this was the so-called fast SCAR) and another to match that of the 5-inch AR (the slow SCAR). As experience in pilot training accumulated, however, it became clear that if adequate sighting tables were available, this range of velocities was not necessary. Interim pilot production of SCAR's by the Institute — a total of 100,000 rounds, completed in June 1944 — included both velocities, but the Bureau of Ordnance production, which was reported to be scheduled at the rate of 500,000 rounds per month,[4] was concentrated, after January 1945, on the fast SCAR. So that pilots in land training could more easily spot ground impacts, the Bureau incorporated into its production rounds a modified head which emitted a smoke puff upon hitting the ground.

Launchers presented no particular problem with the SCAR. With the lug-band button suspension, they could be launched from the slotted rails in the same fashion as the full-size rockets. For the zero-length launchers, it was necessary to develop an adapter rail. This was a shorter slotted rail which could be attached to the posts of the zero-length installations, and from which the SCAR could be fired, just as from the standard, full-size rail.

## Tiny Tim

Development of the aircraft rockets already described gave Navy aircraft a much greater striking power than they had had before. For area bombardment and the destruction of heavy structures, aircraft rockets could not compete with bombs or heavy artillery fire, but for small targets such as submarines, shipping, antiaircraft gun positions, ammunition and oil storage dumps, tanks, and locomotives, they enabled pilots to make hits with the equivalent of 5-inch shells. This fire power and accuracy, plus the comparative ease with which rockets could be mounted on and

[4]Bureau of Ordnance production of SCAR's reached the impressive total of 5,000,000 rounds.

fired from aircraft, inevitably led to speculation about the possible usefulness of larger aircraft rockets.

Lauritsen suggested that a large rocket which could carry a much greater load of high explosive might be useful as a weapon against heavier shipping and against warships larger than destroyers. For such work it would be a supplement to the aerial torpedo but not a substitute for it. This proposed rocket could not compete with the torpedo in the amount of high explosive carried, but it could be fired from higher altitudes and at longer ranges than the torpedo could be dropped.

Preliminary discussions were held among representatives of the Commander in Chief, United States Fleet, the Navy Bureau of Ordnance, NDRC, and the California Institute group. The general consensus was that such a project was worth exploring, although for the time being the Navy would not formally request the development.

These preliminary speculations and discussions crystallized into action on February 24, 1944. On that date, representatives of the Projectile, Propellant, and Production sections of the California Institute group agreed on tentative specifications for a "really big rocket." The most significant details were the diameter of the motor tube, which was to be about 12 inches, and the propellant charge, which was to consist of four solventless extruded ballistite grains, each weighing about 40 pounds.

The choice of a multigrain motor was dictated by two considerations. First, no press then in operation was large enough to extrude a single propellant grain of sufficient diameter for a 12-inch motor tube. Second, if such a grain could be extruded, its web thickness would be so great as to give an undesirably long burning time. A multigrain motor had two disadvantages: additional design problems involved in designing supports to hold the grains properly spaced in the motor tube; and weight penalties, since the grain supports would involve a certain amount of weight that must be subtracted from propellant or pay load. Under the circumstances, however, these disadvantages had to be accepted as the penalty of bigness.

Work on the big rocket began at once. The first step was the development of the motor. The propellant grains were cruciform, very similar to the grain used for the HVAR, but longer. The total propellant weight was 149 pounds. When one of the complete four-grain motors was fired in one of the static-firing bays at Eaton Canyon the blast lifted the concrete roof off the walls and flattened the walls outward. After that, only single grains were statically fired in the Canyon; full motors got their static tests in the open at the Naval Ordnance Test Station, Inyokern.

Sufficient progress had been made by late April so that on the 26th a complete round was fired from a 15-foot ground launcher at Inyokern. This had the standard motor but a light (300-pound) head, which gave the projectile a total weight of 800 pounds. Like the previous aircraft rockets, this

one was stabilized by four fins attached to the rear part of the motor tube. This ground firing was altogether satisfactory; the rocket flew nicely, with no yaw and no noticeable tip-off as it left the launcher.

Since the HVAR had been nicknamed, it was inevitable that the big rocket should undergo the same treatment. Perhaps it was inevitable too that it should be nicknamed Tiny Tim. At any rate, that is the nickname it got and the name it commonly went by, both in the California Institute group and in the Navy. Its official designation was the 11.75-inch aircraft rocket; the dimension was the outside diameter of head and motor tube.

After firings from the ground launcher, the next step was to determine the blast effect of the rocket on airplane structure. Since the blast would obviously be much greater than any hitherto involved in aircraft rocket development, this stage of the testing was carried out with particular care and thoroughness. Rounds were fired at various spacings beneath the bomb bay of a TBF (Avenger). A time-worn TBF, minus one wing, was mounted on its side on the ground, and a firing was made from beside the fuselage, with the bomb-bay doors open. An F6F wing was mounted vertically, held in place by a heavy steel frame to which it was attached at the folding axis of the wing, at the same support points used in the actual aircraft. A round was fired at closer spacing to the wing than any hitherto used. Other firings were made from under a TBF fuselage to test blast effects on the propeller. The result of all these tests was the general conclusion that the round could be safely fired from a TBF with the spacing provided by the launching gear then under consideration.

So far, nothing has been said about how Tiny Tim was to be launched from an aircraft. The means hitherto used for launching aircraft rockets — the slotted rails or the zero-length launching posts — would not do. They were not sturdy enough; and, what is more important, they provided insufficient spacing between the lower wing surface and the round to prevent blast damage. The problem of launching had been considered while the rocket was under development, and by the time the first design had been stabilized, plans for a launcher had been pretty well formulated.

There were three possible ways of launching the rocket: (1) a fixed launcher installation attached either to a wing or the fuselage; (2) a lanyard drop, in which the round would be attached to wing or fuselage, dropped free for firing, and then fired by lanyard control when it was a safe distance below the plane; (3) a displacing gear which would lower the round the required distance for firing and then retract.

The first possibility, the fixed installation, was rejected because to give the round sufficient clearance from the wing or the fuselage would involve a launcher hung so far below the wing that it would create an impossible amount of drag, not to mention probably involving a redesign of the landing gear, to provide clearance between the plane and the ground.

The second, the lanyard drop, was temporarily ruled out at the start because of the belief that it would make the sighting problem very difficult and probably result in high dispersion.

The third, displacing gear, had the disadvantage of involving somewhat complicated apparatus and extra weight. Nevertheless, it was chosen for the first development, chiefly for the reason that it would hold the round fixed in relation to the plane at the moment of firing and also because the TBF (Avenger) could hold both round and launcher in the bomb bay with the bomb-bay doors closed. Thus the problem of extra drag was eliminated, except during the brief interval when the doors opened, the displacing gear swung the round down for firing, and then retracted to its original position within the bomb bay.

The displacement gear consisted essentially of two tubular frames, each pivoted at the upper end to supports within the bomb bay. The rocket was attached to the lower ends of the frames, which were also pivoted. In the up position, the frames, with the rocket attached to the lower ends, were retracted into the bomb bay. When released, the frames were pulled down by the weight of the round. At the bottom of the swing, with the round fixed in angular relation to the flight line of the plane, the firing switch connection was made automatically, the propellant was ignited, and the rocket flew free of the displacing gear, and the latter retracted into the bomb bay.

The first model of this launcher was test-fired at Inyokern. For testing purposes, the displacement gear was mounted in a discarded TBF fuselage, which was elevated sufficiently from the ground to give clearance for the gear in the down, or firing position. The results of this test were good: the displacement gear functioned as expected, and the firing of the round caused no damage to launcher or fuselage.

The next step was to install the launcher in a complete TBF. When this was done, the plane was mounted on a ramp about twelve feet high, so that the rocket would be released about eight feet above the ground. Again there was a series of tests, first with the dropping of inert rounds to check the functioning of the displacement gear, then the firing of live rounds to check the functioning of the firing mechanism and the blast effect on the aircraft structure. Again, the results were satisfactory, and the way was clear for the final test — the firing of the rocket from the plane in flight.

While the launcher development had been under way, various modifications and improvements had been made in the rocket itself, and by the time the displacement launcher was ready for firing from a plane in flight, the rocket design had been sufficiently stabilized for lot production. The big rocket which had thus resulted from the work begun in February had a motor tube 11.75 inches in outside diameter, an over-all length of 123

inches, and a total weight of 1284 pounds. The head, a semi-armor-piercing bomb, with the base modified for attachment to the motor, had a total weight of 590 pounds and carried 150 pounds of TNT. The motor consisted of four cruciform ballistite grains weighing a total of 146 pounds. As in the 5-inch HVAR, a multinozzle arrangement was used, only the number of nozzles was increased to 24. These were spaced symmetrically around a central nozzle with a blowout disk which released when the motor pressure reached 2250 pounds per square inch. The velocity of the rocket was about 800 feet per second, exclusive of airplane speed. (Eventually other models were developed; one had about the same velocity, but the length was reduced to 117 inches and the weight to 1261 pounds; another — the so-called light model, was 114¾ inches long, weighed 1169 pounds, and had a velocity of about 900 feet per second.

By June 22, the launcher was ready and the rocket had been approved for air firing. The culminating tests were scheduled for that date; two dummy rounds were to be dropped from the TBF in flight as a final check on the displacing gear. Then, if all went well, two Tiny Tims were to be fired on two subsequent flights. The pilot of the TBF was Lieutenant Commander (now Commander) Thomas F. Pollock, U.S.N., who had already done the first flight firings of the 3.5-inch AR and the 5-inch AR.

Late that day (June 22, 1943), Lauritsen, who was in Washington on rocket business, received the following telegram:

> HAVE THIS DAY GIVEN BIRTH TO TWIN TINY TIMS WITHOUT ADVERSE EFFECT.
>
> (signed) *Mother Pollock*

Motion pictures of the air firings, taken from a companion plane, were flown to Washington the next day. Official action was taken immediately to assign the highest priority to the further development of Tiny Tim and associated launchers, for the purpose of getting the rocket into Service use as soon as possible. The reason for accelerating the program was the belief that Tiny Tim might be an effective weapon to use in destroying the launching sites in France from which the German V-1 robot bombs were being sent against England. The program was outlined in a memorandum from the Navy Chief of Staff to the Vice-Chief of Naval Operations on June 28, 1944. Work was to start immediately on development of a prototype of displacement-gear launcher installations for the F4U and the F6F aircraft. These were preferred to the TBF because, as fighter planes, they had higher speed and greater maneuverability. If, however, the launching gear could not be satisfactorily accommodated by either of the fighters, then the installation was to be continued for the TBM (the TBM is the General Motors production of the TBF) and NDRC, through the California Institute group, was to provide 50 launchers.

Army Air Forces were to send to Inyokern one aircraft of the type best fitted to carry Tiny Tim, for development of a prototype for a launcher installation. The Institute group would, of course, work with Inyokern in all the launcher developments and in addition would undertake production of Tiny Tim motors at a predicted rate of 5 per day. The Bureau of Ordnance was to modify 500-pound semi-armor-piercing bombs for rocket heads at a rate matching the motor production. The Bureau of Aeronautics was to provide wiring systems and give every possible assistance to NDRC in the procurement of launchers. A few days later the Bureau of Aeronautics formally requested NDRC (which meant in this case the California Institute group) to undertake the development of Tiny Tim launchers for the following aircraft: TBM, SB2C, F6F, F4U—a request which in essence simply added the SB2C (Curtiss Helldiver) to the types of aircraft under consideration.

The heat was on, and again the fullest co-operation between Navy and the civilians carried the job ahead rapidly. The displacement gear type of launcher proved to be suitable for the F4U, though minor modifications had to be made because the bomb bay was not large enough to accommodate the gear and the rocket. Hence, the launcher was designed to retract against the under side of the fuselage, with the rocket partly in the bay. By July 20, the routine of preliminary testing and ground firing had been completed, and six Tiny Tims had been satisfactorily air-fired. As early as July 7, the Navy had requested launchers for 60 F4U's, for the purpose of equipping Marine Air Group 51 "for an urgent project." On July 13, rocket production was requested at a rate which, allowing a deduction of motors for testing, would make a total of 10 motors per day available for Bureau of Ordnance distribution to the using services, this rate to be maintained until delivery of a total of 1500 motors.

Army Air Forces had supplied an A-20, for which the launcher was designed to be as similar as possible to that of the F4U, with the maximum number of interchangeable parts. After ground and air firing late in July and during August, it was reluctantly concluded that the displacement launcher was not feasible for the A-20. Tests had revealed considerable blast damage to the elevators, and the weight involved in strengthening and protecting the elevators would unbalance the entire, already critical control system. Launcher installations for other Army Air Force aircraft were not undertaken until some time later.

In the meantime, considerable experimental work had also been done with rounds fired from the wing of several types of aircraft, and also with drop launching. Two drawbacks had appeared in displacement-gear launching. The first was the interval of 0.8 second between the release of the gear for its downward swing, and the firing of the round, a delay which might result in inaccurate firing if the pilot failed to hold the plane steady

on the target. The second was the bucking of the plane caused by the shift in weight as the round was swung down and the abrupt stopping of the gear as the firing position was reached. The net result of this was also to introduce an element of error into the aiming of the rocket. Wing launching and lanyard-drop launching offered ways of avoiding this difficulty, as well as using Tiny Tim from aircraft which did not lend themselves readily to installation of the displacement gear.

Of these two alternative launching methods, the lanyard drop seemed preferable. The installation was simpler; the rocket could be secured to standard external wing or bomb racks, or within standard bomb bays. And experimental drops with inert rounds showed that the nosing down and yaw of the rocket during the first few feet of free fall were not as great as had been anticipated, and that the rate and angle of free fall were uniform for any given air speed, and dive angle. Hence, the sighting problem for accurate fire would not be unduly difficult. By the middle of August, the lanyard launching work had been pretty well concentrated on the SB2C (Curtiss Helldiver).

After about August 7, the whole Tiny Tim project proceeded at a somewhat less hectic pace. On that date the NDRC was notified by the Bureau of Ordnance that the highest priority for procurement of Tiny Tim for emergency use had been temporarily deferred. (The reason, of course, was the success of the Allied armies in France; the robot-bomb launching sites, if not already captured, were clearly going to be secured before Tiny Tim could be put into action.) Production of motors for emergency use was therefore to stop, but the production of as many motors as might be necessary to perfect the design of the rocket and launcher was to proceed at high priority. To minimize interference with other rocket developments then under way, the Institute was asked not to use more than 1800 pounds of ballistite per day for the extrusion of Tiny Tim motors.[5]

A few days later, on August 10, the following objectives were set up for the decelerated Tiny Tim program. The Navy wished to test Tiny Tim in Service use and set November 1, 1944 as a desirable target date. To meet this target date the Institute group should standardize for preliminary Service production a model of Tiny Tim which they considered satisfactory, and having a safe temperature range of —20° F. to 110° or 120° F., and accumulate a stock of at least 500 of these motors. They should continue tests of different types of launcher, and determine as early as possible which

[5]This limit gives, by implication, a suggestion of the magnitude to which the work of the California Institute group had grown since early in 1942. Toward the end of February 1942, Sage and Lacey reported extrusion operations at the rate of 200 pounds of powder per day for the whole rocket-development program.

combination of launcher and aircraft should be used for the Service test. Highest priority should be given to determining the ballistics of launching from the gear finally selected. Improvements of the (then) current head and fuze should be continued, and tests inaugurated on other fuzes and on heads designed to give optimum underwater trajectory.

This program outlines pretty completely the work that was carried on for the remainder of 1944. Late in August, the part of the work involving air firing was slowed up by a tragic accident. On August 17 and 18, two Tiny Tims were successfully fired from an SB2C by lanyard drop. On August 21, two more firings were scheduled. The first was made without any unforeseen results. On the second, the rocket dropped clear; the lanyard firing functioned perfectly, with the proper time interval (about .04 second), and the proper displacement of the round below the plane (approximately 3 feet); and the rocket had a normal flight. But almost immediately after the firing, the plane, which was at about 1500 feet altitude, nosed over into a very steep dive, and crashed, killing the pilot, Lieutenant John Murray Armitage, U.S.N.R.[6]

Further air firing of Tiny Tim was immediately suspended. The SB2C was so completely demolished that it provided no evidence as to the cause of the disaster. The obvious inference, however, was that some hitherto undisclosed blast effect was responsible. Therefore, steps were immediately taken to make an intensive study of blast effect not only on the SB2C but on other types of aircraft from which Tiny Tim was fired.

New and elaborate test setups were devised, with instrumentation for much more exact and discriminating measurement of the forces exerted on the aircraft structure when the rocket was fired. The result of all this investigation was the conclusion that a very severe initial shock wave was caused immediately after ignition by the blast from the igniter — a shock wave of much greater magnitude than that caused by the blast from the burning propellant grain. In the case of the SB2C, it seemed clear that the igniter shock wave had jerked an elevator trim tab free of the metal restraining clips and locked it in the position which caused the fatal dive. In addition, the igniter blast caused considerable damage to the tail structure and surfaces.

The first result of all these investigations was, as might be expected, a reduction in the black-powder charge of the Tiny Tim igniter. Through a series of tests it was found that an igniter containing slightly less than a fifth the original amount of powder (235 grains instead of 1200) would eliminate the dangerous shock wave and would still ignite the propellant grains satisfactorily. A restudy was also made of the distance the rocket

[6]As a memorial to him, the experimental air center at the Naval Ordnance Test Station, Inyokern, was named Armitage Field.

should drop free from the plane before being fired, and this distance was increased. Recommendations were made with regard to strengthening the parts of the airplane structure and protecting the surfaces which were affected by the blast.

Air firings were resumed in late September and early October. Work after that consisted in further improvements in rocket and launcher. The support for the propellant grains was redesigned, giving better burning characteristics. Fuze functioning was improved by providing three base fuzes instead of one. A sphere-ogive head was designed to give the rocket a better underwater trajectory. The lanyard-drop launcher was adapted to several other types of aircraft. Along with this developmental work went pilot training and air firing to accumulate data for sighting tables.

Beginning late in 1944, Army Air Forces undertook a program of outfitting appropriate planes for firing Tiny Tims. This program, as it shaped up, would have had the planes ready for action in the invasion of the Japanese home islands.

## Aircraft Rocket Sights

The aiming of barrage rockets is relatively simple. The boat or vehicle on which the launchers are mounted is pointed at the target area. A preliminary shot or two establishes the proper range; and then the barrage rockets are fired in salvo, their normal dispersion accomplishing the saturation of the target area.

The aiming of forward-fired aircraft rockets is another matter. Since the plane speed added to their own cuts their dispersion down, they are accurate enough to be fired at point targets. Since the launchers are fixed in position on the plane, the problem of sighting is really a problem of aiming the plane. But it is a relatively complicated problem, because many factors are involved.

Sighting for accurate forward firing of aircraft rockets must take into account the following: altitude, indicated air speed, and dive angle of the plane, and slant range from the target. The effects of altitude, air speed, dive angle, and slant range on the trajectory of any given type of aircraft rockets are constants, and can be determined by calculation and verified by range firing. Hence, pilots can be supplied with sighting tables which will tell them what sight setting they need for the particular plane and rocket involved, at any given combinations of practicable air speeds, dive angles, and slant ranges.

Suppose the pilot is flying an F4U, its eight zero-length launchers loaded with 5-inch HVAR's; his attack is to be made at a 60° dive angle, with an air speed of 320 knots and a slant range of 1500 yards. Consultation of his F4U sighting tables will tell him what adjustment to give his sight, so that with the pipper of the sight on the target and the speed, range, and

dive-angle conditions fulfilled, he can be reasonably sure of scoring a hit.

As aircraft rockets went into Service use, the first great necessity was to prepare such sighting tables. The California Institute group, in conjunction with the Navy, undertook this job, the Navy supplying the planes, pilots, and range officers, and the Institute group collecting the range data and reducing them to tabular form. In the course of this work, hundreds, even thousands, of aircraft rockets were fired, mostly on the air-firing ranges at the Naval Ordnance Test Station, Inyokern; and the editorial section of the California Institute group worked under forced draft to get the results into print and the tables started on their way to the fighting fronts. The great majority of aircraft rockets fired in combat from Navy planes were aimed by means of these sighting tables.

Satisfactory as the sighting tables were in providing the necessary aiming data, there were certain practical difficulties in using them. The sights had to be set for preselected constants, whereas conditions at the moment of the attack might make variations desirable. Even if the first attack was carried out at the speed, dive angle, and slant range determined upon, conditions of subsequent attacks — a different type of target, for instance, or a variation in the amount of antiaircraft fire — might make a change in dive angle or slant range desirable. A resetting of the sight, or an aiming allowance, imposed a rather heavy burden on the pilot, who, under the circumstances, was likely to have plenty to keep him occupied without that. There was also the difficulty of estimating the slant range and of maintaining the selected dive angle. Training in rocket firing and continued combat experience, of course, gave the pilot greater proficiency, and the magnificent record made by aircraft rockets in combat is testimony enough to the skill and proficiency of the pilots.

Nevertheless, it was obvious that a sight which had an automatic input of, and response to, some or all of these elements of altitude, speed, dive angle, and slant range would be highly desirable, not only in freeing the pilot of some of his worries but probably also in raising the level of performance.

As far as the National Defense Research Committee was concerned, the development of such a sight was the business of Division 7, which was organized to work on problems of fire control. By arrangement with Division 7, a special section of the California Institute group was organized to work on aircraft-rocket sights, under the direction of H. W. Babcock.[7] They developed the first aircraft-rocket sight to go into Service use, their so-called Type 2.

This was not a completely automatic sight, but it was a good start for one. Target altitude, dive angle, air speed, and powder temperature are

[7]A member of the staff of the Mount Wilson Observatory. He had joined the rocket group much earlier.

hand-set; altitude is automatically fed into the sight from a barometric altimeter. Thus, changes in altitude automatically and continuously readjust the sight, permitting the pilot to fire at any range within the effective limits of the ammunition. Two different types of ammunition may be fired, if need be, on the same pass, by the positioning of double-throw switch.

For combat evaluation, 16 F4U aircraft of a squadron based on the carrier *Gilbert Islands* were equipped with this sight. During the period May 29 to July 3, 1945, this squadron carried out 46 strikes and support missions involving a total of 231 flights. Because of combat and operational losses, the number of F4U's varied during this period, but a minimum of 13 aircraft were kept equipped with sights.

J. L. Fuller, a member of Babcock's group, went out on the *Gilbert Islands* with this squadron as observer and technical adviser. The following summary is based on his report and the report of the commanding officer of the carrier.

There is not space enough here to give the details of this trial period. For present purposes the significant thing is the performance of the sight. It fulfilled admirably the hopes and expectations of its designers. The pilots had little difficulty in learning to operate it, since the principles involved are not much different from those learned in standard rocket training. Maintenance of the equipment was found to be relatively easy, since customary radio-repair methods were followed, whereby precalibrated units were substituted for defective units — a process requiring, on the average, only about five minutes. The radio-repair shop on the carrier was found to be completely adequate in facilities and trained personnel for making all the necessary repairs.

The sight proved to be stable under operating conditions, an important factor because of the small amount of time available for adjustment and repair. Some units were in operation for as long as three weeks, with no attention whatsoever, and that under severe weather and operational conditions. If a sight was in operation at all, it was giving reliable sight settings.

Analysis of mission reports indicated that the accuracy of the sight in combat use compares very favorably with the results obtained during tests at the Naval Ordnance Test Station, Inyokern, and that the expectation of 60 per cent of the 5-inch HVAR's would hit within ten mils of the aiming point was not contradicted. Pilots agreed that this computing sight made possible a far higher percentage of hits in combat than could be secured with the previous fixed sighting methods.

The California Institute produced for the Navy, with NDRC approval, a total of 100 of these Type 2 sights.

But it is with sights as with rockets — once you've developed a good

one, you want to develop a better one. And so the aircraft-rocket sight section went on to develop their Type 3 and Type 4 sights.

The Type 3, which was tested at Inyokern in the spring of 1945, differed from the Type 2 in that it added automatic dive-angle compensation effective for attack approaches made at any dive angle between 15° and 50°. Air speed still had to be set manually by the pilot. The first test model of Type 3 was made for use with the 5-inch HVAR only, but later the sight was modified so that it could be used with either that rocket or Tiny Tim, the selection being made by a double-throw toggle switch. It could also be converted, by adjusting a single lever, into a fixed sight for machine guns.

The tests of the Type 3 sight demonstrated it to be satisfactory, but it was never put into quantity production. While Type 3 was being developed, the sighting group was working on Type 4. The latter included all the features of Type 3, plus automatic input of air speed. Progress on Type 4 was so rapid that there was no need of putting Type 3 into production. Type 4 was very nearly completed when the war ended; the sighting group worked on a few weeks and finished it up — a very satisfactory conclusion to their activities.

## Training

No weapon, however well developed and proved, is a success until it is well-used. The primary responsibility for training in the use of any weapon devolves of course upon the using Service. But with new weapons such as radar or rockets, the NDRC had a responsibility to lend the maximum of assistance in training. The California Institute was no exception to this rule. Although the Navy undertook the primary responsibility for the training and the full story is therefore a Navy story, it should be pointed out that among others John McMorris[8] of the Institute group served as adviser to Fleet Air West Coast in all its rocket activities, which pioneered aircraft-rocket training in the country. He did outstanding work in helping to outline training procedures and develop training aids. He took a very active part in the training activities both on the East and West Coasts. This was cut off by his tragic death on May 5, 1944, in an airplane crash en route from Quonset to Nantucket.

## Combat Use

The 3.5-inch aircraft rockets went into Service use in the winter of 1943–44. The first submarine kill which can be credited to them was made

[8]He held a Doctor's degree in Chemistry from the Institute. At the time the group began its work, he was teaching in the Chemistry Department of Pasadena Junior College. He took leave of absence to join the Institute group, where he headed a subsection on special problems.

in the Atlantic on January 11, 1944. Two carrier-based TBF's surprised a German U-boat on the surface. They began the attack with a rocket-firing run, the second plane getting two probable and two certain hits. Then they made a second run to drop depth bombs, and one of the planes got a perfect straddle. Since the U-boat was still surfaced and circling slowly one plane bored in again to machine-gun the flak gun crews. As the second plane followed, the U-boat began to submerge, and the second plane dropped depth charges squarely over it just as the conning tower went under. Shortly afterward, the U-boat emerged with the bow up at a 50-degree angle, then settled level in the water and began to circle slowly again. Then bow and stern began bobbing up and down alternately. Soon it went dead in the water, and a large puff of smoke blew out of it, apparently from an internal explosion. Immediately afterward it sank stern first, for good.

Aircraft rockets first went into combat in the Pacific on February 15, 1944. Although Squadron VC-7, the first to be trained in aircraft rocket firing by Commander Fleet Air West Coast, had gone out to the fleet in late December 1943, it was a Marine squadron — VMTB-134, flying TBF's — which had the distinction of firing the first American aircraft rockets at the Japanese in the Pacific.

That this squadron carried off the pioneering honors was due to their own enterprise and the ingenuity of a Service Squadron. Somehow — the explanation lies, no doubt, somewhere in the mysteries of the supply system — enough launching rails to equip twenty planes were sent out to the Southwest Pacific. They fell into the hands of Marine Squadron VMTB-134, Major Alben C. Robertson, Commanding Officer. There was nothing but the launching rails and a general idea of what they were used for — no instructions for installation and wiring, none for sighting, and no rockets. But the squadron decided to install the launchers anyway; rockets would doubtless come along later. The actual installations were made by the Service Squadron on Espiritu Santo Island, Captain Ferris, Commanding Officer, with Colonel F. J. O'Neil, Strike Commander, Commander Air, Solomons, pushing the project. Installing the eight launcher rails on the first plane, with no guide but cut-and-try and common sense, took three days and 300 man-hours of work; by the time the last installations were made, the rate had been reduced to sixteen man-hours per plane.

But there were still no rockets. A cargo ship carrying a supply was finally located. On direct orders from Admiral Halsey it was rerouted to bring rockets to the squadron. The rockets, of course, proved to be stowed away in one of the more inaccessible parts of the hold. Rather than unload a good part of the cargo, a bulkhead was cut through. The rockets — 3.5-inch and 5-inch AR's — were finally pulled out and sent to the squadron,

which, by this time, had moved up to Bougainville. The rockets reached them on February 8, 1944. Since a strike was scheduled for a week later, there was little time for training—only three days, February 10, 11, and 12. On February 15 five planes of the squadron took part in a daylight shipping strike at Keravia Bay in the Rabaul area of New Britain Island. Coming down to their attack through heavy antiaircraft fire, they fired their rockets at practically point-blank range (500 to 600 feet). Two of the pilots pulled out of their attack run before they could see whether they scored hits, but reported that they didn't believe they could have possibly missed. Two other pilots saw their rockets hit. One of them, describing his attack run, said, "Coming out of the dive, I sighted on the ship's side and let the rockets go . . . a second later I pulled the bomb lever. The rockets and bombs smacked the ship like the old one-two in boxing."

Although Holy Moses (the 5-inch High Velocity Aircraft Rocket—HVAR) was developed primarily for, and in close co-operation with, the Navy, a set of special circumstances gave an Army Air Force squadron the honor of first using the rocket in combat.

In the spring and early summer of 1944 England was undergoing the V-1 robot-bomb attack. While antiaircraft barrage and airplane countermeasures succeeded in preventing all but a small percentage of the robot bombs from getting through to their target, the more fundamental strategy was to dispose of the problem by eliminating the launching sites, especially those in the Pas-de-Calais area. For this purpose it was thought that the 5-inch HVAR might prove to be an effective weapon. Therefore, in June 1944 arrangements were made to send a special mission to England to equip a squadron for Service-testing of this new rocket. The members of this mission were: Lieutenant Colonel (now Colonel) Harry L. Donicht, who was head of the section of the AAF Material Command at Wright Field which had the responsibility for aircraft work; Paul Reichert of Wright Field; Lieutenant Colonel T. W. Hornsby of Eglin Field; C. C. Lauritsen and Carl D. Anderson of the California Institute group; and Group Captain H. W. Dean of the Royal Air Force. This group flew to England to supervise launcher installation and pilot training. The California Institute, with NDRC approval, agreed to furnish 1900 rounds of the new rocket; the first 1400 rounds were to be produced at the rate of 100 rounds per day and flown to England; the last 500 rounds were to be sent by ship. Thomas Lauritsen, back in Pasadena, successfully kept production and shipments up to this schedule.

The squadron chosen to pioneer the 5-inch HVAR in the European Theater of Operations was the 513th Fighter Squadron (SE), 406th Fighter Group, Ninth Air Force, AAF. By the time the installation and training were under way, it appeared doubtful whether the rockets would be able to do any significant damage to the launching sites; as these proved

to be so sturdily built of reinforced concrete that they could withstand even heavy bomb hits. Nevertheless, the preparation and rockets were not wasted. When the Allied Forces made their push out of the Normandy beachhead, the squadron went operational on July 15, 1944, flying their P-47's from their British base to attack targets in the St.-Lô area.

The squadron had had only limited training — limited both in time and in number of rockets fired. In fact, one or two pilots had not fired a rocket at all before they took off that day. The installations were limited also; each aircraft had launchers for only 4 rockets instead of the 8 which Navy planes mounted. In spite of these handicaps, however, the squadron chalked up a splendid record, which reached its peak on July 25 and 26. On those two days, the squadron flew a total of 64 sorties. They destroyed 12 tanks, hit 13 (one of which was left afire), destroyed one truck, one armored car, one half-track, one pillbox, rocketed flak towers, and reported on three occasions (as if to explain the lack of a better score) that "suitable targets were lacking."

In a letter dated August 30, 1944, Lieutenant General Carl Spaatz, Commanding General, United States Strategic Air Forces in Europe, highly commended the mission which enabled this squadron to be equipped, trained, and placed on operational duty in two and one-half weeks, and stated that "the success of the equipment has resulted in a requirement from the Ninth Air Force to equip all of their P-47 fighter aircraft with rockets." Early the following January, Major General B. E. Meyers, Deputy Director, Air Technical Service Command, commending NDRC for this mission and for subsequent co-operation, characterized the 5-inch HVAR as "the best anti-tank weapon of the war."

This was a fine start. But the rest of the story, as far as the use of HVAR's by the AAF is concerned, rather fizzles out. Part of the difficulty may have been due to the fact that the pioneering squadron made an excellent record with only negligible training, and so no adequate and comprehensive training program was set up for pilots in the European Theater of Operations. The 513th Fighter Squadron, however, was an exceptional group, and their performance should not have been assumed as a norm for pilots in general. Another difficulty was bad co-ordination of supply. A large number of planes were equipped with zero-length launchers for firing 5-inch HVAR's (one story is that 600 P-47's were so equipped by the summer of 1945). A large supply of rockets was sent both to England and to France, the Navy having allocated 40 per cent of its HVAR production to the AAF. But somehow, rockets and planes failed to be in the necessary places at the necessary times. P-47's firing HVAR's helped break the German counteroffensive in August 1944; otherwise, HVAR's played only a minor and sporadic part in subsequent AAF operations in the European theater.

The story has already been told of the first submarine kill with aircraft rockets in the Atlantic. There, and in the Mediterranean, aircraft rockets continued to be used chiefly as armament for planes engaged in antisubmarine search and patrol-carrier-based TBF's and TBM's (Avengers) and land-based PV-1's (Venturas).

There was, however, one notable exception. Rocket-firing F6F's based on aircraft carriers were provided by the Navy for the invasion of Southern France in August 1944. These F6F's not only supported the initial landing operations; flying from their carrier bases they continued to provide air support for two weeks as our troops drove the German forces north. Their operations during the period August 15 to 29, 1944 were credited with playing a large part in the demoralization of German transport. They finally ceased operations only because of the rapidity of the German retreat and the lack of targets.

Naval operations in the Pacific, however, involved by far the greatest use of aircraft rockets, both in the number of rounds fired and in the variety of missions which rocket-firing planes were called on to perform.

Details have already been given of the first use of aircraft rockets in the Pacific — the strike on Rabaul by Major Robertson's Marine squadron, flying TBF's equipped with rail launchers and armed with 3.5-inch and 5-inch forward-firing rockets. It will be recalled that the original motive in the forward-firing development was to furnish an additional weapon for antisubmarine warfare, that the 3.5-inch aircraft rocket and the rail launchers were developed for this purpose, and that the first planes equipped were TBF's and TBM's, the kind used in antisubmarine operations.

Hence, in the desire to get the new weapon into wider use as quickly as possible, the first equipment to reach the Pacific was rail launchers, and the first squadrons to go out were trained on TBF's. Actually, antisubmarine activities in the Pacific constituted only a very small part of the work that rocket-firing planes were called on to do. Their principal work was carrier strikes, attacks on Japanese shipping, support of ground forces in landing and subsequent operations. TBF's, already heavily loaded, were comparatively slow, and the additional drag of the rail launchers further cut down their speed and hence their effective range. The fullest potentialities of aircraft rockets were not realized until other types of planes, scout and dive bombers and particularly fighters, equipped with zero-length launchers, and a supply of 5-inch HVAR's began flowing westward. This does not detract from the splendid service performed by rocket-firing TBF's during the interval before rocket-equipped SB2C's, F4U's, and F6F's went into action.

Early experience with aircraft rockets in the Pacific taught several useful lessons. The first was that aircraft rockets were most effective against point targets — antiaircraft gun positions, ammunition and oil-storage dumps,

planes in revetments, shipping; to attempt to make them do the work of bombs was to waste them. The second lesson was that, as a rule, better results came from making separate attack runs for rocket firing and bombing. The third was that rockets with delay fuzes were needed. The first 5-inch aircraft rockets sent to the Pacific had only instantaneous nose fuzes. These were effective against personnel, but with many targets, pillboxes, shipping, etc., it was desirable to have the rocket penetrate before it detonated. This was a problem for the development group.

Rockets with a delay base fuze were supplied. Later, it was found that the most satisfactory solution was to provide each rocket with an instantaneous nose fuze and a delay base fuze; when the target was of such a nature that penetration before detonation was required, the rocket could be fired with the nose fuze safe — that is, incapable of arming.

From early in 1944 on, there was a constantly increasing flow of rocket-trained squadrons, and a corresponding flow of equipment and rockets. As zero-length launchers became available, they were installed on planes in forward areas, and, in this country, as post-production "fixes" after the planes came from the manufacturers. One naval facility, for instance, in December 1944 reported having made 200 such installations, and was scheduled to make over 400 more. As rapidly as possible, rocket equipment — launchers, firing systems, etc. — was incorporated into current production. By February 1945, the Chief of Naval Operations stated in a communication to a long list of commands and facilities that "all carrier based and twin engine land combatant type aircraft (are being) delivered by the contractors fully equipped to fire rockets."

Improved and new fuzes were put into production and fed into the supply lines as rapidly as possible. The same was true of improved rockets and of the new 5-inch HVAR. As fast planes with zero-length launchers went into service, and later as the HVAR's reached the fleet (probably about December 1944) the reports of rocket effectiveness increased, as did the enthusiasm for this new weapon. One task force commander, reporting successful long-range fighter sweeps in the Philippines, by planes armed with 5-inch aircraft rockets, concluded, "With the receipt of high velocity rockets and rockets of greater size it is considered that rockets will become the primary offensive weapon of aircraft." Or this report from fighter squadron: "Considerable use was made of rockets . . . and it is the unanimous opinion of all pilots that they are one of the most effective weapons we can employ. . . . The Air Group Commander would be willing to carry 12 rockets per plane if there were launching posts for them." Vice-Admiral John Sidney McCain, in a magazine article,[9] explained how rockets augmented the striking power of the fleet for the Mindoro invasion. Fighter squadron strength was increased on the carriers by putting ashore dive

[9] "So We Hit Them in the Belly," *Saturday Evening Post,* July 14, 1945.

bombers and replacing them with F4U's (Corsairs) and F6F's (Hellcats), both of which were armed with rockets or 1000-pound bombs. He points out that with wheels lowered to act as diving brakes, they could begin to match the punch and accuracy of the dive bombers in the latter's specialized field, precision attacks on ground targets and enemy shipping. This change, he continues, involved no great sacrifice in striking power, and gave carrier aircraft a new flexibility, for after the Corsairs and Hellcats had expended their bombs and rockets on ships and ground installations, "they could revert to purely fighter status, and air groups would then have a deadliness they never had before."

A few excerpts from combat reports will give an idea of the day-to-day work of aircraft rockets. These are not unique instances; they are selected only as typical examples. The first shows the results of rocket fire against shipping:

> An attack transport (5400 tons) was attacked with rockets by a TBF. Two hits were made at the waterline and two amidships. The ship was burning fiercely in the stern and the bridge was ablaze from the rocket hits and subsequent strafing. She was not seen by later strikes and probably sank.
>
> A smaller attack transport had two gaping holes put in her port side by rockets. She was left dead in the water and is believed to have been one of the abandoned hulks seen on a search the next day.
>
> Rocket hits on a small attack transport (2800–3000 tons) started a fire in the stern that enveloped a space of 40 feet. This ship was sunk.
>
> It is believed by this command that the rockets were extremely effective. . . . One pilot who fired two rockets from about 300 feet directly into the starboard side of an attack transport reported that the rockets entered the side and then went off, blowing a hole outwards, the size of which was apparently ten to fifteen feet in diameter.

The next excerpt illustrates the effectiveness of rocket fire in silencing antiaircraft batteries:

> The devastating effect of rockets against the antiaircraft batteries and upper decks of one naval auxiliary was well demonstrated. This particular vessel, just prior to this attack, had shot down one medium bomber, killed a man in another, and fired with such accuracy at other bombers that none of their bombs hit the target, which was at anchor. Two other aircraft attacked this vessel fore and aft with sixteen rockets of which thirteen were hits. . . . Small fires were started on deck, upper works were blown about, and no more AA was observed from that vessel on that day or the succeeding day. . . .

The carriers *Monterey, Cowpens,* and *Belleau Wood* reported their fighter craft successful on long-range fighter sweeps carrying 5-inch rockets, and added, "The enemy ships destroyed at Iloilo, Zemboanga, Coron, and Cebu by these fighters are ample proof of this success." Rocket-firing planes from the *Enterprise* even succeeded in sinking a Japanese destroyer.

Other combat reports list such items as the following: an armored car on railroad tracks blown up by four rockets; a radar position knocked out by two 100-pound bombs and four rockets; thirty-two aircraft destroyed on Apari airfield; strafing and rockets the most effective means of destroying planes in revetments at Clark Field. The carrier *Marcus Island* reported that in strikes against Palau, the Celebes, Morotai, and the Philippines (August 29–October 1, 1944) her TBM's fired a total of 740 rockets (708 five-inch and 32 solid-head 3.5-inch); performance reported by pilots was "uniformly excellent"; the rockets were "quite effective particularly against tanks, oil storages, ammunition dumps, pillboxes. . . . The *Hornet* reporting the first operation in which she sent out rocket-firing F6F's, stated that the rockets proved to be a very effective weapon against a variety of targets, but principally ships and AA positions." A task group reported its conclusion that "the rocket fighter is the best aerial weapon against land batteries of AA guns."

As operations in the Pacific proceeded on an accelerating scale, new uses were found for aircraft rockets in support of ground troops. The role of aircraft rockets in dislodging the enemy from well-fortified positions and in the support of ground troops is described in an account prepared by the Bureau of Aeronautics:

> . . . here the rocket was used more widely than in any other application and was ideally suited to its task. When our forces landed . . . they found the Jap burrowed in well-protected caves, block-houses, and pill-boxes. Sufficient protection was provided so that only the heaviest bomb proved effective in dislodging the Japs from these positions, and the problem was doubly complicated by the Japs' effective camouflage which made it impossible to detect these positions except at low altitudes which exposed the bombers to lethal antiaircraft fire. In addition, targets were so small and so located that direct hits were hard to obtain and it was found necessary to saturate any particular area to insure hits. . . . This required heavy concentration of bombers and subsequently toll of aircraft and personnel.
>
> A change in tactics was indicated and a new munition capable of greater penetration than the average bomb and accurate enough for use against pinpoint targets was required. The aircraft rocket filled this requirement better than any known weapon and its use in this field alone more than justified the many months of development work and money expended.
>
> Effective and close liaison was set up between the ground forces and the carrier forces supporting them. Wave after wave of rocket-equipped fighters flew in at low altitude, aiming their rockets with deadly accuracy into the heretofore invulnerable Japanese positions that were taking such a toll of our advancing ground forces. Dive bombers . . . silenced gun positions with their deadly rockets, allowing them to release their bombs at low altitude safely and demolish the target with the heavier destructive power of the bomb. The ground forces soon learned the value of this new weapon and the rocket planes were called upon

whenever tough obstacles presented themselves. An air co-ordinator, armed with rockets with smoke heads, was kept over the target continuously during the strike. Radio contact with the ground forces indicated the targets which were giving them the most trouble and which they wanted wiped out. The air co-ordinator marshalled his rocket planes, made an approach with smoke-head rockets to indicate the exact target and left the rest to the planes armed with rockets with their high explosive heads. The result was usually a "Well done—Thanks" from the ground forces and a subsequent advance made a great deal easier by the destructive damage of the aircraft rocket.

A typical report from a carrier supplies an example:

"Rocket equipped VT (i.e., torpedo planes) attached to this ship were used against caves and camouflaged strong points only able to be seen from low altitude. The accuracy was excellent and the damage inflicted on points of resistance by just a few rockets was often sufficient to clear the way for an advance. Ground observation parties were extremely well satisfied with the results obtained."

The intricate and complicated cave systems of Iwo Jima and Okinawa were in part a tribute to the effectiveness of this type of attack.

Thus as the war in the Pacific proceeded, aircraft rockets were called on to play an ever-increasing part. By the summer of 1945 the pattern for effective use was well defined; manufacturers were turning out rocket-equipped planes in great quantity; and the Bureau of Ordnance's rocket production had reached the point where the limiting factor was storage facilities for completed rounds.[10]

The first unit to be trained for firing Tiny Tim, the 11.75-inch aircraft rocket, was Marine Air Group 51, flying F4U's equipped with displacement gear launchers. They finished their training at Inyokern early in January 1945. They did not, however, go into Service, probably because of a decision from higher echelons that the displacement launcher was not satisfactory and that lanyard-drop launching method should be used.

Finally, for Service test, Tiny Tim squadrons flying F4U's were sent out in the spring of 1945 on carriers *Franklin* and *Intrepid*. The *Franklin* was a part of Task Force 58 which was in Japanese waters on March 18. The following day the *Franklin* was cruising only a little over forty miles off shore. At 7.08 A.M. a Mitsubishi two-engine bomber slipped through the combat air patrol, made its run over the *Franklin* from bow to stern, and released two 500-pound bombs. Both hit the flight deck along the centerline, one-third and two-thirds of the way aft. Both were armor-piercing. They penetrated the flight deck and exploded against the hangar deck, which was filled with planes fueled and armed and warming up ready to be taken to the flight deck.

[10]Total production of 5-inch aircraft rockets was something over a million rounds; an equal number of 5-inch HVAR's were ready by V-J Day for the invasion of Japan.

The story of the resulting holocaust and the heroic efforts by which the *Franklin* was kept afloat is too well known to need retelling here. In the inferno of the hangar deck were the F4U's armed with Tiny Tims, fourteen of which, set off by the flames, added to the general destruction of planes, material and men.[11]

The Tiny Tim squadron of the *Intrepid* went into action at Okinawa but the amount of naval gunfire, artillery fire, and general rocketing was so great that it was impossible to make any satisfactory evaluation of the effectiveness of the big rockets.

The end of the war left Tiny Tim as a potentially powerful and effective weapon which would enable a plane to deliver the punch of a twelve-inch gun, but a weapon which never had a thorough combat test of its capabilities.[12]

[11] A full account of the *Franklin* disaster is given in *Great Pacific Victory* by Gilbert Cant, pp. 388 *ff*.

[12] The war ended before the most formidable rocket plane got into action against the Japanese. This was the F4U equipped to carry eight 5-inch HVAR's and two Tiny Tims — a total of 3800 pounds of potential destruction.

# CHAPTER XXII

# *SUMMER 1944*

IN THE SUMMER and early fall of 1944 the work of the California Institute group was at its peak — in number of personnel involved, in extent and variety of range activities, and in production of rockets for development and test and for interim Service use. The last major rocket-development program of the group was in full swing. And finally, arrangements were made and facilities provided for continuing rocket work under Navy auspices when the end of the war should take NDRC out of the picture. While major projects went on, there were other things to do; no full understanding of what a big NDRC program was like is possible unless we digress to mention some of them.

## Demolition Rockets and the Demolition Duck

In the summer of 1943 the Army became interested in a short-range demolition rocket which, carrying a large loading of high explosive, could be fired from tanks to knock out pillboxes, breach concrete and masonry walls. The 7.2-inch rocket which the Institute group had already developed as an antisubmarine weapon (i.e., the Mousetrap round, with a light-case head carrying 30 to 35 pounds of HE) was suitable as far as general characteristics were concerned. Hence, rather than embark on the development of a new rocket, the Mousetrap round was adapted for demolition purposes, the principal change being in fuzing. General requirements called for a launcher which could be tank-mounted. A design was suggested and a first model built by the Engineers Board of the Army. Then, following this preliminary work, the launcher section of the Institute built an improved model. After tests on the West Coast, Gould took it to Fort Pierce, Florida, where further tests were made.

The launcher, which had a capacity of twenty demolition rockets, was mounted on the front of an M4 tank. From its general appearance and the fact that it was mounted low on the tank, it was nicknamed the cowcatcher. The Fort Pierce tests were on the whole satisfactory, though like most tests they suggested further improvements. The rocket was standardized by the Army, which later supplanted the cowcatcher with other launchers designed for tank mounting.

In the fall of 1943, the Institute group was requested to undertake a special project involving demolition rockets. This request came from a

special NDRC committee on Demolition of Landing Obstacles, set up to evaluate and develop proposed weapons for the invasion of Europe, which was already in the planning stage.

What Committee DOLOC wanted was the 2½ ton 6 x 6 amphibious truck Duck equipped for firing demolition rockets; they believed that this equipment would be useful in clearing underwater and beach obstacles as the invasion landing was made.

NDRC assigned this project to the California Institute group. Work was begun in December 1943 and carried through on high priority. The launcher design consisted of two sets of rails, twenty-one in each set, giving the Duck a launcher capacity of forty-two rounds of demolition rockets. The launchers were designed so that they could be installed in or demounted from the Duck easily and quickly.

Demolition Ducks, with this launcher equipment, were tested for the Army and for the Joint Army-Navy Board at Fort Pierce, Florida. The results of all these tests and demonstrations were completely satisfactory, and there was every expectation that a Service requirement would be forthcoming. In the meantime, in order to meet an exigent deadline for invasion materiel, the Institution group, at the request of the chairman of Committee DOLOC, and with NDRC authorization, procured launcher equipment for 100 Ducks.

Unfortunately, however, the story of the demolition Ducks deteriorates into anticlimax. The expected requirement for D–Day never materialized, and when the war ended the launchers were still warehoused, waiting for a requirement.

The 7.2-inch Mousetrap round modified for demolition purposes was also standardized by the Navy. Fired from landing craft, it could be used for clearing shallow water and beaches of mines, barbed wire, and concrete or log obstacles without exposing demolition teams to direct enemy machine-gun fire. The Porcupine, a 32-round launcher for gunwale mounting, which was developed late in 1943 by the joint Army-Navy Experimental Test Board, formed the basis of the Navy's version of a launcher. By February 1944, the "Woofus" had been developed. This was a 120-rail launcher (fifteen rows of eight rails) mounted in the cargo space of an LCM(3). By varying the angle of elevation of the rails, a shot pattern could be obtained extending from 180 to 280 yards from the craft.

LCM's with Woofus launcher installations were first used in the invasion of Southern France in August 1944. Twenty-one of them deployed just ahead of the first landing wave, firing their rockets at H minus 2 minutes.

There was no other occasion in the European or Mediterranean Theaters to use the LCM(Woofus). Subsequent production of rounds and launchers went to the Pacific, where they were used principally in connection with the work of the underwater demolition units.

## WINDOW

So-called Window is a countermeasure to radar. Window consists essentially of a pay load of metal-foil strips carried aloft by an artillery shell or rocket. At a predetermined range and altitude a time-delay mechanism operates to eject these metal-foil strips, which produce a clouded, blurry image on the radar scope. Hence, they are sometimes called radar muddler.

The Bureau of Ordnance and the Bureau of Ships of the Navy Department inaugurated the development of using rockets to project Window. After initial tests showed the feasibility the Bureau of Ships, which was to supply Window to the fleet, requested the Bureau of Ordnance to supply experimental rockets for further tests. After successful completion of this experimental lot, the Bureau of Ordnance requested the Jet Propulsion Laboratory at the Naval Powder Factory to develop a standard rocket for ejecting Window and to produce a number of units for further test. This project was completed early in April 1944; the resulting Window rocket had a 3.25-inch motor and a 3.25-inch ejector head. The Window consisted of 3000 metallized strips of paper one-fourth-inch wide and of varying lengths.

In the meantime, the California Institute group was requested to develop the design for a Window rocket loaded with two pounds of metal-foil strips. The design was completed quickly; on March 2 the group was authorized to start production on 25,000 Window rockets "for extensive proving in the fleet." The design was subsequently modified, but was considered complete by July.

The Institute's Window rockets have a 3.25-inch motor very similar to that of the 3.5-inch aircraft rocket. The 3.5-inch head may contain as many as 76,800 metallized strips three-sixteenths of an inch wide and varying in length from one-half inch to sixteen inches, the differing lengths affecting different wave lengths of radar. These strips are ejected approximately at the peak of the rocket's trajectory. The group was responsible for two models. The first ejected its load of Window at approximately 900 yards' altitude and 1800 yards' range; the other at approximately 700 yards' altitude and 2000 yards' range. The Institute produced for the Navy about 12,000 rounds of the first and about 16,000 of the second.

The launcher consisted of a standard supporting six 7.5-foot T-slot rails, paired to form three I-beams. This launcher could fire not only Window rockets but 3.5-inch and 5-inch aircraft rockets as well. It was used for the ship-to-shore firing of 5-inch AR's at Okinawa, which will be described in the next chapter.

The first operational firing of Window rockets occurred in the Normandy invasion, when they were issued for use against enemy fire-control radar not already knocked out by bombardment. In Southern France, PT boats and

converted crash boats equipped with launchers fired Window successfully as a countermeasure to German radar. On D–Day plus two, PT boats screening a task force used Window rockets to baffle enemy searchlights and guns. After they fired their Window, the enemy shifted his searchlights and guns to the Window cloud, having interpreted its image on the screen as an air attack coming in. In Cherbourg harbor, a minesweeper operating in broad daylight was put under fire by coastal batteries. He fired about twenty Window rockets and soon saw the pattern of fire move away from his ship. A few minutes later, the Window cloud having fallen, he was again put under fire. Again he fired Window rockets, and in the ensuing confusion of the battery fire was able to withdraw from the danger zone.

Over 30,000 Window rockets were shipped to the Pacific Fleet, intended primarily for firing from amphibious craft. Whether they were so used is doubtful. The Seventh Fleet, for instance, had fired none by March 1945 and at that time did not contemplate using them. Whether they would have proved useful in the invasion of Japan is another of the questions which the end of the war left to conjecture.

## The Rocket School

In November 1943, the Chief of the Bureau of Ordnance wrote to the Chief of Naval Personnel pointing out that naval activities in combat areas require officers with extensive knowledge of rocket-type ammunition and launchers, and that during the next few months, because of the anticipated increase in the use of rockets, there would be a much greater demand for such personnel. Therefore, in order to have rocket-trained officers available, he requested the Bureau of Personnel to arrange for the necessary instruction, which should include "classroom work, observation, and practical work in the function, installation, maintenance and capabilities of rocket launchers." He recommended that the Bureau of Personnel negotiate a contract for this training with the California Institute to start about December 15, 1943.

After considerable discussion in Pasadena, the final arrangements for the school, as suggested by Admiral Holmes, were set up as follows. The Rocket School was to be operated as a Service school under naval command, preferably at the Naval Ordnance Test Station, Inyokern. Since Inyokern, however, was not at that time sufficiently advanced to permit setting up the school there, the first classes were to report to the Liaison Officer at the California Institute, where the NDRC group, as part of their activities under OSRD Contract OEMsr-418 would outline and supervise the course of instruction.

The first class convened at the Institute early in January 1944 for a four-week course. This course was planned to cover "all phases of rocket development which have been adopted for Service use" and "to stress the

practical side" so that the students would qualify "as rocket gunnery officers afloat or ashore." Instruction was carried on not only at the Institute but also at nearby ranges and naval facilities.

At the Institute, the course covered the following subjects: descriptions of projectiles and launchers developed by the Institute group; propellants, including properties and methods of manufacture and extrusion; internal ballistics of rocket motors; external ballistics; inspection of the Eaton Canyon propellant plant with loading of static test motors and explanation of how test data are secured and interpreted; review of rocket fuzes, including practice in taking apart and assembling fuzes.

At the Goldstone range, the students served as members of the range crew, assembling, loading, and firing rounds for the various tests in progress. They also helped in spotting and in computing the resultant data. Then the range was turned over to them and they assembled, loaded, and fired several rounds of each type of Service projectile. In addition, they were given a number of 4.5-inch barrage rockets and were required to determine range and dispersion for rounds fired at 10° F. and at 120° F.

At the Pendleton range, the students assisted in proof firing rounds from production lots. They also worked out an elementary field problem under the direction of the Marine personnel of the Rocket Battalion.

At the Landing Craft School, San Diego, the students took passage in rocket-equipped support boats accompanying a landing exercise in which Marines were put ashore. After this, the support boats put to sea, and firing of live rounds was carried on in seaward areas.

At the Naval Air Station, San Diego, the students were familiarized with various types of aircraft, launchers, and associated equipment, with safety precautions, storage, and special procedures on carriers. They were given practice in loading rounds and testing firing circuits, and in harmonizing launching rails and boresighting rocket sights.

At Inyokern, the students assisted in aircraft-loading operations and witnessed forward-firing tests. (Later, as Inyokern facilities were developed, more of the instruction was given there. At the time the first class was in progress, only the airfield and associated aircraft activities were in operation.)

After the first class, it was found that essentially the same instruction could be compressed into three weeks. The school continued, with the Institute group supervising the course and supplying a part of the instruction, until the end of August 1944, when the Naval Ordnance Test Station, Inyokern, was able to take over. Commander Haugen acted as officer in charge of the Rocket School during these first eight months while the Institute was the center of its activities; he reports that 11 classes went through the school, training a total of 118 officer students and 5 civilians. In addition, three Wave officers, not included in these figures, attended the school

for one week with the class which convened June 26, 1944; they received the instruction given in Pasadena but did not go to the ranges or Naval facilities.

## The Bureau of Ordnance Design Unit

At the same time that the decision was made to establish a rocket range at Camp Pendleton (late in the spring of 1943), the Bureau of Ordnance of the Navy set up a related project for the establishment of a West Coast Drafting Room. By that time several different types of rocket developed by the California Institute group were already in Service use and more were in prospect. In each case, the Institute had carried on crash and interim production of rockets to meet urgent Service requirements until the Bureau of Ordnance could secure production from its prime contractors. It seemed likely, also, that such crash and interim production by the Institute would continue to be needed as new rocket developments were completed.

Drawings offered a difficulty in the change-over from Institute to Bureau of Ordnance production. For fabrication of rocket components, the Institute group depended on local subcontractors in the Los Angeles area, who worked from drawings and specifications supplied by the drafting section.[1] These served the immediate purpose; they did not, however, conform to the general style and practice of Bureau of Ordnance drawings. Furthermore, for changes introduced while Institute production was under way, the pressure of time was often so great that only sketches were made, not finished drawings. The result was that the Bureau of Ordnance, in initiating its own prime contracts, was likely to be delayed because, though the design of a particular rocket was ready to be standardized for production, Bureau of Ordnance drawings still had to be finished and checked — drawings suitable for use as production drawings and for the procurement of materials in production quantities. Contracts might have been negotiated on the basis of Institute drawings; but such an arrangement was obviously unsatisfactory to the Bureau, since it had the final responsibility for the quality and performance of the finished work.

As a means of solving this difficulty, the Bureau of Ordnance made a contract with Walter Dorwin Teague, of New York City, to set up a West Coast Drafting Room. Under the contract the Teague organization was to supply designing and drafting services. At the time this contract was made, the development of launchers for the 4.5-inch barrage rocket had high priority; hence, the services of the Teague organization in connection with launcher work were stressed. Because of the establishment of the Pendleton range and the stationing of an Ordnance Testing Unit there, it was at first believed that the West Coast Drafting Room should be located there.

[1]This section was under the direction of V. F. Ehrgott, who made many valuable contributions in working out design detail and suggesting design simplifications to speed production.

The final choice, however, was left to Mr. Teague. After he had looked the situation over, he decided that the drafting room should be in Pasadena because the whole rocket-development program was centered there at the California Institute. Hence, the West Coast Drafting Room was set up a few blocks from the Institute campus. The Teague organization supplied personnel and supervision; their drawings were sent back to the Bureau of Ordnance in Washington for checking.

By the end of the year, it was apparent that in spite of the establishment of the West Coast Drafting Room, the problem of drawings had by no means been solved. No naval personnel were connected with or attached to the Teague group in Pasadena. When drawings needed corrections, they had to be sent back from Washington to Pasadena, with all the obvious delays in transacting business the parties to which were far apart. Rocket development by the Institute group had mushroomed: during the latter part of 1943 the forward-firing aircraft-rocket program had come very much to the fore, and the development of spin-stabilized rockets was well under way. The net result was that the Bureau of Ordnance was not much better off with regard to drawings of rockets, fuzes, launchers, and associated equipment.

The Bureau decided, consequently, to establish a Bureau of Ordnance Design Unit (BODU) in Pasadena, so that all the necessary functions could be carried out on the spot, with full Bureau authority. BODU began functioning officially on July 1, 1944; the Bureau had begun collecting personnel for it as early as the preceding January. Commander C. E. Haugen was designated officer in charge; he had already become familiar with the rocket work through duty in the office of the Navy Department Liaison Officer at the California Institute. To staff BODU, the Bureau of Ordnance sent West one of its ablest engineers, F. E. Patrick, and three other civilian engineers, and assigned seven other Navy officers to duty there. The Teague group was assimilated into the unit; the contract under which they did the bulk of the drafting work was administered for the Bureau by Commander Haugen.

In order to facilitate liaison with the research and development group, the Navy requested, and OSRD authorized, the Institute on the recommendation of Division 3 to provide working space for BODU. Culbertson Hall on the campus was used. There, until shortly after the end of the war, it carried out its mission of preparing Bureau of Ordnance production drawings, specifications, and ordnance pamphlets for rockets and rocket launchers.

## Technical Observers

In the fall of 1943, OSRD established the Office of Field Service, with the object of facilitating the exchange of information and experience be-

tween the NDRC groups which were developing new weapons and devices and the Service forces which were putting them into operational use. The first member of the California Institute group to go out under the auspices of the Office of Field Service was W. A. Fowler. He arrived in New Caledonia early in March 1944 and during the next two months made the rounds of various commands in the South Pacific and Southwestern Pacific. His itinerary included the headquarters of Admiral W. F. Halsey and the United States Army Forces in South Pacific Area (USAFISPA) in New Caledonia; the Third Amphibious Command at Guadalcanal; the Fourteenth Army Corps and the Thirteenth Air Force at Bougainville; Headquarters, United States Army Forces in the Far East (USAFFE) in Australia; and the Second Engineer Special Brigade in New Guinea.

His mission had a variety of purposes. The most important, perhaps, was to talk with the groups who were already using rockets and get their criticisms of the new weapons. In some cases he was able to make suggestions for more effective use. For example, he passed on the information, based on experimental work back in the States, that the launcher rails for aircraft rockets could be cut down twenty-eight inches, thus reducing the objectionable drag, without any loss in accuracy of fire. In other cases, he could send back to the group in Pasadena suggestions for improvements and new developments, the need of which had been shown by Service experience. Pilots of rocket-firing planes, for instance, wanted a better sight; and aircraft rockets, to function effectively against armored shipping, should be provided with a delay fuze.

At Guadalcanal, where the Third Amphibious Command was equipping landing craft with gravity-feed automatic launchers for barrage-rocket firing, he was able to help with launcher installations and suggest training procedures. Wherever he went, he brought the Service forces up to date on the most recent rocket developments by showing motion pictures taken by Commander Gilks and his camera crew, and distributing descriptive pamphlets and manuals prepared by the editorial section of the Institute group. In some cases, he could suggest uses for rockets which had not yet reached the Pacific — the 7.2-inch demolition rocket, for example, to destroy underwater obstacles and Japanese pillboxes. He also carried the word of new developments like the zero-length launchers and the 5-inch HVAR, which were then available or soon would be.

Fowler's experience showed the need of just such liaison between the fighting forces and the research and development groups as technical observers could supply. Consequently, in his final report to the chief of the Office of Field Service he strongly recommended that the sending out of technical observers be extended and made a regular procedure.

After Fowler, the Institute group supplied a succession of technical ob-

servers. All went to the Pacific, and all followed the general pattern of Fowler's activities: visits to various commands; advice and assistance in any current rocket problems; dissemination of information concerning new rocket developments; and recommendation to the group in Pasadena of developments the need of which was indicated by Service experience.

The next technical observer to go out after Fowler was L. A. Richards, head of the Land and Amphibious Launcher Section. He left in midsummer, 1944, and was gone for five months. He was followed by P. E. Lloyd, who was also from the Land and Amphibious Launcher Section. Lloyd was sent out toward the end of January 1945. He, too, was gone for about five months. The tide of American military success had by that time swept up into the Philippines, so that he was able to include an inspection of Japanese rocket equipment captured in the Philippines and to help make arrangements for sending specimens of Japanese rocket propellant back to Pasadena for analysis.

In mid-July two more men were sent out: R. E. Sears, also from the Land and Amphibious Launcher group; and R. V. Adams, supervisor of the Aircraft Ballistics group at Inyokern, whose primary concern was to be aircraft rockets and the preparation then under way by the Army Air Force to use them in the invasion of Japan. Both reached the Philippines before the war ended. Sears was on Samar; Adams was on Leyte at the Headquarters of the Thirteenth Air Force. The end of the war ended their missions, and they were recalled to the United States. L. H. Mahony, whom the OFS had assigned to the Institute group for rocket training before sending him to the Pacific as a technical observer, left in August 1945. He had got only as far as Hawaii when the war ended, and he, too, was recalled to the States.

Only one member of the Institute group was sent as a technical observer or adviser to the European Theater of Operations. This was A. L. Melzian, assistant supervisor for administration of Anderson's Aircraft and Ballistics Section, who was attached, not to the Office of Field Service but to the office of the Secretary of War. Like Sears and Adams, his activities were short-lived. He reached the European Theater only a short time before V–E Day, and returned to the States immediately afterward.[2]

## SUMMER ACTIVITIES

By the summer of 1944, then, the Rocket School and the Bureau of Ordnance Design Unit were domiciled on the Institute campus. The torpedo-

[2]This section omits mention of members of the Institute group who were sent abroad on special missions: Lauritsen and Anderson to England and France in connection with the first use of the 5-inch HVAR; Sears to the Hawaiian Islands to help in readying LSM(R)'s firing spin-stabilized rockets; and Fuller on the *Gilbert Islands* for Service test of the Type 2 aircraft-rocket sight. Accounts of these missions are given elsewhere in connection with accounts of the rockets or equipment involved.

launching station was in operation. Work in Pasadena was being more closely co-ordinated with the needs of the using Services by the technical observers who were going out under the auspices of the Office of Field Service.

Two rocket programs were going ahead at high priority during the summer. The first — forward-firing aircraft rockets — has already been discussed in the preceding chapter. The particular press during the summer was for further refinements and crash production of the 5-inch HVAR, and for completing the development of Tiny Tim. The second rocket program comprised the series of spin-stabilized rockets, which will be discussed in the following chapter.

With these two rocket-development programs proceeding concurrently, and both under the pressure of urgency, there was, as might be expected, a corresponding pressure on closely related sections of the Institute group. The Fuze group had new problems to solve with each new type of rocket. The Propellant Section was called on for new motor designs as well as for production of propellant grains, igniters, etc. The Experimental Development sections, in order to take care of anticipated production requirements, began during the summer the building of a new plant on the outskirts of Pasadena. The Personnel Section carried on special campaigns to meet the increased need for workers. The Editorial Section assumed an extra load in doing the computing and preparing the sighting tables for aircraft rockets; and in order to facilitate the spread of information about the current work of the Institute group, supplemented its weekly progress reports with a semimonthly *Confidential Bulletin*.

The ranges were correspondingly busy. Goldstone was used to capacity. At Pendleton, work was scheduled regularly for both land and sea ranges. When the Marine Corps' rocket-training program at Pendleton was terminated at the end of the summer, there was, consequently, a decrease in range activity there. The work at Inyokern, however, more than made up for the drop at Pendleton. The Inyokern development was so extensive, and had such significant relations with the future of rocket development, that it calls for discussion in a section of its own.

## Inyokern

The genesis of the Naval Ordnance Test Station (NOTS) at Inyokern, California, has been mentioned in an earlier chapter. The decision to establish the station was made and the site was selected during the summer of 1943. From the start, the Bureau of Ordnance planned NOTS as a permanent facility. Although the immediate emphasis of the work there was to be upon rockets, the Bureau of Ordnance wanted the station to be able ultimately to take care of practically any type of ordnance testing. Therefore, several additional tracts of land were added to the original site, until

the station included an area about the size of the State of Rhode Island. As plans for the station matured, the decision was made to build there a pilot plant for rocket propellant (i.e., dry-extrusion presses and processing lines), and to provide staff and facilities for research and development work.

The California Institute group co-operated to the fullest extent with the Bureau of Ordnance in planning and development of NOTS. This co-operation had the immediate object of providing facilities and personnel for current rocket work, and the long-range object of getting the station staffed and equipped to continue rocket research and development after the close of the war should bring NDRC-sponsored activities to an end.

In the early fall of 1943, Lauritsen and others of the Institute group took part in the reconnaissance of the tract reserved for the station, advising on the location of ranges, experimental areas, etc.

The most urgent need was facilities for aircraft-rocket work. A representative of the Bureau of Aeronautics who had come West to witness tests at Goldstone in August 1943 reported the tests as "eminently successful," but pointed out the need for an adequate range for test firing and development, with facilities for servicing planes and housing personnel. For aircraft-rocket work, planes had to be flown in to Goldstone. The dry lake bed, which was the range, had to serve also as a landing field. Consequently, the range work with other types of rockets had to be interrupted; and there were no hangars for planes or adequate equipment for making repairs and adjustments. As the aircraft-rocket program gathered momentum a steadily increasing amount of aircraft firing was required, not only for developmental work but also to accumulate data for sighting tables.

Hence, the first stage in the development of the new station was to provide facilities for aircraft-rocket work. Just outside the village of Inyokern (and not included in the area set aside for the station), Kern County maintained a small emergency landing field. This the Bureau of Ordnance leased and developed by improving the flight strips, building hangars and shops, and providing housing for personnel. At the same time, ranges for air firing were laid out, cleared, and marked, and aircraft-rocket work was well under way before the close of the year.

In the meantime, the permanent headquarters area of the station, about nine miles east of Inyokern, was being laid out, and temporary housing, mess hall, office building, warehouses, etc., were being rushed to completion. Until they were ready, the airfield, which had been designated Harvey Field, served as headquarters; and the first members of the Institute group who were stationed at NOTS lived at Harvey Field. During the fall of 1943, Lieutenant Commander (later Commander) C. E. Haugen, U.S.N. (Ret.), who was already in the office of the Navy Department Liaison Officer at the Institute, served as Bureau of Ordnance Representative for

Inyokern. At about the end of the year, Captain S. E. Burroughs, Jr., U.S.N., took over as commanding officer of the station.

The story of Inyokern's development during the next year — how a community of several thousand mushroomed in the desert — is a fascinating one, and it is a pity that there is not space enough to tell it here. The essential fact, however, is that in spite of the obvious difficulties temporary facilities were set up which took care of the immediate necessities of the rocket program, and these were replaced as rapidly as possible, without interfering with the essential work, by permanent installations which would enable the station to fulfill the long-term mission.

By the summer of 1944, in addition to Harvey Field, the following facilities were in operation: aircraft and ground ranges for live and inert firing; a plate range for testing fuze action; an experimental area where most of the work on Tiny Tim was carried on and replicas of Japanese pillboxes and bunkers were built to test the effectiveness of rocket fire; reinforced concrete walls for fuze and penetration tests; storage magazines for rocket heads, motors, and fuzes. For work on underwater trajectory of aircraft rockets, the station secured the use of Haiwee Reservoir, one of the storage units of the Los Angeles city water system, some forty miles north of Inyokern. Another airfield, which was to be the center for experimental work, was under construction, and the first unit of the propellant plant was nearing completion.[3]

To facilitate the work at Inyokern, the Institute group established early in 1944 a new section, Inyokern Range Operations, headed by Emory L. Ellis,[4] and consisting of the following groups: Terminal Ballistics, Aircraft Ballistics, Motor Ballistics, Computing and Instrumentation, and later, Photography.

This section, the members of which were stationed at Inyokern, served several purposes. Besides organizing and directing range operations, they were able to train Navy personnel in various phases of range work, and they supplied the nucleus of a civilian organization which, under Civil Service, could continue after the Institute's rocket activities ended.

The pilot plant for rocket propellant came into the Inyokern picture fairly early. The need for such a plant, as an augmentation of the Eaton Canyon facilities, had become apparent during the summer of 1943. The Eaton Canyon tract, however, was already rather densely utilized. It might have been possible to acquire more land there for the pilot plant, but if postwar operation was contemplated, there would be difficulty because Eaton Canyon was close to residential areas. The propellant plant was

[3]Some idea of the magnitude of the construction activity can be got from the fact that through the summer of 1944 the contractor maintained a civilian crew of about 6000 men.

[4]He had taken a Doctor's degree from the Institute some years before. He had joined the rocket group early in its activities, and before going to Inyokern had headed a subsection in charge of motor testing and later, a range operations group.

tolerated there as a wartime necessity, but there would be vigorous objections to continuing operations there after the emergency was over.

Location of the pilot plant was still undecided when the Navy settled on the Inyokern area for its new Naval Ordnance Test Station. Then it appeared that there would be obvious advantages in building the pilot plant on the station, particularly in view of the fact that the Navy proposed to continue rocket research and development there after the war. Hence, at the Navy's request, and with NDRC approval, the Institute group undertook the design of the plant, the procurement of equipment, and the supervision of operations through an adequate run-in period.

The burden of carrying out this commitment fell principally upon the Propellant and Interior Ballistics Section of the Institute group, of which Sage, Lacey, and Clark were joint supervisors. The plant was to consist of a twelve-inch and an eighteen-inch dry-extrusion press, each with its own processing line. The area set aside for the plant was some two and a half miles east of the headquarters area of the station and separated from it by a low ridge of hills. This area was large enough for the two presses and lines, with enough extra space for an additional twelve-inch press with processing line if there should be need of it later. The whole installation was designated the China Lake Pilot Plant, named for a dry lake near by.

Detailed preliminary plans for the China Lake Plant were prepared by the building design group of the Propellant Section, of which Palmer Sabin, a Pasadena architect, was the head. On the basis of these plans, final construction drawings were prepared for the Propellant Section by the consulting engineering firm of O. G. Bowen, of Los Angeles. Construction work, which was under the cognizance of the Bureau of Yards and Docks, was carried on by the Macco Construction Company, who were the contractors for the whole station, under direct contract with the Navy. Thus, the Institute group had the responsibility for the planning, and shared with the Navy the responsibility for inspection as the construction proceeded. The Institute group also had the primary responsibility for procuring and installing the equipment for the press buildings, the processing lines, and the static-firing bays.

Plans were drawn up in the fall of 1943 and the winter of 1944. Construction work began in the spring of 1944. As work on the China Lake Plant got under way, the Propellant Section set up its own organization at Inyokern. During the spring and summer of 1944, the primary purpose of this Inyokern branch was to discharge the section's responsibilities involved during construction; it was headed by an administrative assistant and comprised pay roll, operational control, and engineering groups.

The twelve-inch press and processing line were the first to be completed. They went into operation early in November 1944. As these propellant-production facilities became available, the Inyokern activities of the Pro-

pellant Section expanded correspondingly, and to take care of staffing and operating the China Lake Plant, the Inyokern branch was enlarged by the addition of eight more groups: technical control, experimental production control and shipping, inspection, safety, personnel, material control and stores, first-aid, and maintenance.

Like the section set up at Inyokern for range operations, the Inyokern branch of the Propellant Section not only provided a means of carrying on effectively the expanded work of the Institute group but also furnished an organizational framework for continuing an important part of that work when, with the end of the war, the Institute returned to its normal peacetime activities.[5]

[5]The eighteen-inch press and processing line were completed but not in operation before the end of the war.

# CHAPTER XXIII

## *THE SPINNER FAMILY*

WE HAVE seen earlier that there are two feasible methods of stabilizing rockets in flight. One consists in attaching longitudinal fins which will exert a steering and restoring force to keep the rocket headed in the desired direction. The other method consists in causing the rocket to rotate rapidly about its longitudinal axis so that the gyroscopic forces thus set up will stabilize it in flight as they stabilize a conventional rifle bullet or artillery shell.

British rockets were fin-stabilized. Since the California Institute group, when they began their work in the fall of 1941, were following the lead of the rocket work already done in England, they designed their rockets for fin stabilization. And they continued with fin stabilization through the series of forward-firing aircraft rockets already described.

Fin-stabilized rockets, however, except when fired from aircraft, have a relatively high dispersion. That is, at a given range and launcher elevation, a salvo of rockets can be depended upon to fall within a given area, but hitting a point target within that area is wholly a matter of chance. To be sure, if enough rockets were fired, the random distribution of impact points would probably ensure a hit on the point target eventually. But that is a highly inefficient way of going after point targets.

For area saturation, such as landing barrages and preparation for an advance of ground forces, this dispersion is acceptable, as was seen in the case of 4.5-inch barrage rocket, though even here the dispersion could have been cut down to advantage. Many situations could be envisaged, however, in both land and amphibious warfare, where a rocket capable of more accurate fire could be used effectively because of the lightness and portability of the launcher.

Stabilization by spin, on the other hand, offered possibilities for greater accuracy. (Later work, in fact, demonstrated that the dispersion of spin-stabilized rockets is relatively unaffected by launcher length.)

O. C. Wilson, a member of the Projectile Design Section,[1] carried on for a short time some preliminary experimental work on a small spin-stabilized rocket with a motor 1.25 inches in diameter. But because of the press of other rocket developments then under way — the Chemical Warfare rocket,

[1]Of the staff of the Mount Wilson Observatory. At that time he headed a subsection which was concerned with problems of accuracy.

the target rocket, and the Mousetrap round — and because of the shortage of sheet ballistite, this exploratory work was dropped. It was not until over a year later that interest in spin-stabilized rockets was renewed, and the California Institute group decided to go ahead with spinner development.

This decision may have been hastened by the knowledge that the Germans had put spin-stabilized rockets into Service use. For stimulating interest in a new weapon, there is nothing like finding out that the enemy is already using it.

## The 3.5-Inch Spin-Stabilized Rocket

The California Institute group's work on fin-stabilized rockets was indorsed and defined by a specific request that they develop a spinner which might be substituted for the 75-mm. pack howitzer by the Marine Corps ground forces. This requirement dictated the approximate size of the rocket, which was expected to do the work of the howitzer shell. The first model was fired on the Goldstone range. The development followed the usual process of successive modification and test until the final design had the following characteristics. The head, 3.5 inches in diameter, carries 1½ pounds of TNT. The motor diameter is 3.25 inches; the propellant, 2½ pounds of ballistite dry-extruded in a cruciform grain. The stabilizing spin is imparted to the rocket by the use of 8 nozzles set in the nozzle plate at a cant of 12°. The over-all length of the rocket is about 24½ inches; its total weight, 24 pounds. The maximum range is about 4000 yards; the velocity, about 750 feet per second.

Concurrently with the 3.5-inch, a 2.25-inch spinner was developed. This was intended primarily for experimental purposes; no Service application was ever made.

While work was proceeding on the 3.5-inch spinner, a parallel launcher development was being carried on. Several different types of launchers were engineered, one for experimental firing to gather data on fundamental characteristics of spin-stabilized rockets, several others to meet possible Service requirements. One of these launchers was a 14-round gravity-feed automatic patterned after the barrage-rocket automatic. Another, with a gravity side feed, had a capacity of 6 rounds. For the possible Marine Corps use of the rocket as a replacement of the 75-mm. pack howitzer, a single-barreled launcher was developed for mounting on a standard machine-gun tripod. With a total weight of only 26 pounds, it was well adapted to use in rough and difficult terrain.

The difficulty, however, was that while the 3.5-inch spinner was more accurate than a corresponding finned rocket could have been, its dispersion was still too great to make it a clean-cut competitor with the 75-mm. pack howitzer. This was the sort of situation that arises often enough in the de-

velopment of new weapons, particularly when the new weapon is not a supplement to an old one, but a potential replacement. The new one has certain advantages: in the case of the 3.5-inch spinner and the single-barreled launcher, the obvious advantage was great mobility because of light weight. But in other respects, the new weapon compares unfavorably with the old (as the 3.5-inch spinner did with respect to dispersion). In such a situation, the substitution of the new weapon for the old is hardly justified; and from the practical point of view of supply, there is no clear justification to add another weapon and ammunition. This was the reason why no Service requirement for the 3.5-inch spinner ever developed from the Marine Corps, though the California Institute produced, upon Navy request, 10,000 of the rockets.

The 3.5-inch spinner came to life, however, in another connection during the last few months of the war. Another division of the National Defense Research Committee was requested by the Office of Strategic Services to develop a very lightweight launcher for this rocket. This development was satisfactorily carried out, and 3.5-inch rockets were requisitioned from the supply produced by the California Institute.

Which of the varied and mysterious activities of the Office of Strategic Services was to be armed with the 3.5-inch spinner was a matter for conjecture. Whatever the plan, it never came to fruition, for the war ended with the rockets at a port of embarkation awaiting shipment overseas.

## The 5-Inch High-Velocity Spin-Stabilized Rocket

The next development in the spinner family was undertaken to fill a very definite need. The development was successfully completed, and the rocket went into action.

This was the 5-inch high-velocity spinner (5-inch HVSR). It was developed to provide PT boats with greater fire power. As early as August 1943 advices had begun coming back from the Pacific that the Japanese were using armored and armed barges to supply and reinforce island garrisons from a central base, and that the standard PT boat armament was not particularly effective against these barges. Various countermeasures were suggested: a small fast armored boat specifically designed to counter Japanese barge activities; equipping PT boats with bazookas, or with 75-mm. aircraft guns or with rocket-firing installations. Bazookas were actually tried out, but their range was too short and their dispersion too high.

It will be recalled that late in 1943 the Commander, Motor Torpedo Boat Squadrons, Seventh Fleet, had begun equipping the PT boats in his command with launchers for the 4.5-inch rocket, and that during the following year he had had considerable success with them. Nevertheless, the 4.5-inch barrage rocket, because of its large dispersion and comparatively low

velocity, was far from ideal as a weapon for PT boats. As a stopgap, it performed well while nothing better was available; but the desirability of providing something better was evident.

Even before the 4.5-inch barrage rocket installations for PT boats had begun, however, a suggestion had been made as to the proper rocket armament for PT boats. This suggestion came in the fall of 1943 from Captain John H. Thach, U.S.N., head of the research and development section of the Office of the Commander in Chief, United States Fleet, soon after the development work on the 3.5-inch spinner was under way. If the hopes of greater accuracy with spinners could be realized, then a long-range, high-velocity spinner might well be the answer to PT boat requirements. Such a rocket was not planned for use at the full range; the combination of long range and high velocity would enable it to be used against barges and other shipping with reasonable accuracy in flat-trajectory fire at ranges of 700 to 1000 yards.

The California Institute group therefore began the development of a 5-inch spin-stabilized rocket designed for a range of approximately 10,000 yards. This development went along almost concurrently, with that of the 3.5 spinner, and if the rate of progress was somewhat slow, as compared to that of the aircraft rockets, it must be remembered that with the spinner developments the California Institute group were moving into a comparatively new field, in which the problems were inherently more difficult.

By the summer of 1944 the 5-inch HVSR was ready for sea testing. A squadron of PT boats in process of reassignment from the Aleutians to the Pacific was held for a while at Terminal Island to co-operate with the California Institute group in trial launcher installations and sea firing. Unfortunately, at this juncture the propellant group ran into powder difficulties. Sample grains from the lots extruded in Eaton Canyon had a regrettable tendency to cause motor blowup in static firing. The reason, as it later appeared, was that the sheet ballistite used for these grains was the first made by a simplified process which failed to colloid the materials sufficiently. Until this difficulty could be traced down and corrected, the Applications Safety Committee could not release rockets for sea tests.

The interval of waiting was filled in by working out launcher installations, by bringing the officers of the squadron up to the California Institute for a short rocket indoctrination course, and by practice firing at a towed target with the PT boat's 20-mm. bow gun. For this practice firing, the bow gun was lashed in position and lanyard-fired. A simple vertical bar mounted on the bow served as a sight for the helmsman to keep the PT boat headed at the target (the rockets were to be aimed by aiming the boat); the gunner, stationed beside the helmsman, used an aircraft gunner's sight, firing the bow gun by lanyard when the pipper of the sight was on the target. This exercise provided useful experience in conning the boat, developing team-

work between helmsman and gunner, and determining the best attack speed.

By the time the propellant difficulties were cleared up, most of the squadron had started across the Pacific, leaving behind PT boats 73 and 75 to carry on the sea testing of the rocket. Ammunition was ready late in August, and the final sea-firing tests were concluded on August 23, 1944. On that day, with 73 doing the firing and 75 standing by as an escort and observation boat, some forty-odd rounds of 5-inch HVSR's were fired at a target set back a little from the shore line on the north side of San Clemente Island.

The target was a cloth-covered wooden frame, approximately the length and above-water height of a Japanese barge. The launcher used was one of the type designed especially for PT boat purposes. It consisted of 8 tubes arranged in tiers of four, one above the other, and supported by a deck mount. The launching tube part of the launcher could be revolved inboard for loading and then swung outboard for firing, thus doing away with the necessity of blast shields. The launching tubes could be adjusted to varying degrees of elevation, but were locked in train and pointing ahead when swung outboard for firing. Firing was controlled from a firing box mounted near the gunner's sight.

For these tests, a single launcher was mounted on the starboard side of the forward deck. The complete Service installation, however, consisted of two such launchers, one mounted starboard and one port, giving a capacity of sixteen rounds without reloading.

The concluding test on August 23 was reasonably rigorous, for there was a fairly high sea running and, incidentally, some queasy scientists. The procedure was to make an attack run at 1000 r.p.m. The radar operator gave the range, and the firing began at 1000 to 700 yards, the gunner getting off as many shots as possible during the first 200 to 250 yards of the run. Of the forty-odd rounds fired on August 23, four were direct hits and at least the same number were good ricochet hits.

Tests with another PT boat squadron at Fort Pierce, Florida, a little later in the year, gave better results in terms of hits scored on the target.

The performance of the 5-inch HVSR as a PT boat weapon was considered to be so good that the Bureau of Ordnance initiated extensive procurement of launchers and rounds. Installations were to be made at advanced points, and during March and April 1945 a program of equipping PT boats was carried on in the Philippines.

Two models of the 5-inch HVSR went into Service use. Each has a total weight of about 50 pounds, an over-all length of about 30 inches, and a 10-pound cruciform propellant grain. One, however, has a so-called "common" —that is, semi-armor-piercing type—head, it is base-fuzed, and has a filler of 1.7 pounds of explosive D. Its velocity is 1415 feet per second and

its lateral dispersion 8 mils. The other model has a "general-purpose" — that is, a moderately thick-walled — head, and a point-detonating fuze. Its filler is 2.8 pounds of TNT, its velocity is 1540 feet per second, and its lateral dispersion, in low-angle fire, 7 mils.

## The 5-Inch Spin-Stabilized Barrage Rocket

While the high-velocity spinner was under development, work was also being carried along on other spin-stabilized rockets. The goals for this part of the spinner program were indicated by special Navy plans and requirements. The 4.5-inch barrage rocket, as has been pointed out earlier, was sometimes limited in its effectiveness because of its range limitations of 1000 to 1100 yards. Offshore obstacles such as reefs, etc., might keep rocket-firing LCI's too far offshore for their rocket barrage to accomplish its purpose. This was the case, for instance, during part of the Saipan operation in June 1944. Hence, the Navy was interested in the possibility of a longer-range barrage rocket.

For this purpose, a 5-inch spinner with suitable range seemed to offer the best possibility. Several spinners with 5-inch diameter were already on the stocks (in addition to the high-velocity spinner); and a 12-round gravity-feed automatic launcher had been developed, similar in principle to the 12-round automatic launcher for 4.5-inch rockets. Spinners were not so accurate when fired from this automatic as when fired from tubular launchers, but for barrage purposes this dispersion was not too great. (Eighty per cent of the 5000-yard-range spinners fired from automatic launchers at full range fell within an area 500 by 500 yards).

During the fall of 1944 the Navy drew up plans for a rocket gunboat which was to utilize to the full the potentialities of the spin-stabilized rocket. These plans included the development, by the Bureau of Ordnance, of a continuously reloadable rocket launcher with adjustable elevation and train by director control. This rocket gunboat, which was to be ready by June 1, 1945, was to use the LSM hull, but was otherwise to be designed primarily for mounting 10 of the new launchers, 4 mortars, 1 five-inch gun, and various automatic weapons.

All these plans and considerations resulted in formulation by the Institute group in the fall of 1944 of an integrated program for barrage spinners. In its essentials, this program called for concentration of immediate effort on 5-inch spinners with three different ranges: 5000 yards, 2500 yards, and 1250 yards. All were to have the same weight, about 50 pounds, and the same length (about 32 inches).[2] These common characteristics would make for much greater ease in handling and storage, and would permit the use of

[2] Work on the 5-inch HVSR was also being continued, chiefly in order to reduce its dispersion. But this rocket, while capable of being fired from a slightly modified automatic launcher, was not intended for barrage purposes.

a single type of launcher for all of the barrage spinners. It was further planned that for each of the ranges, the following assortment of heads should be available:

(1) Common. Semi-armor-piercing, with explosive D loading and with base fuze;
(2) General-Purpose. A moderately thick-wall shell (about ½ inch) with TNT loading and nose fuze;
(3) High-Capacity. A thin-wall shell (about ¼ inch) with maximum TNT loading and nose fuze;
(4) Smoke. A very light-wall shell (about ⅛ inch) with either WP or FS filling, a nose fuze, and a tetryl burster;
(5) Chemical Warfare. Similar to the smoke head, but designed for filling with chemical agents of densities 1.43 or less;
(6) Pyrotechnic. A light-wall shell with time fuze and separating charge to eject an illuminating flare and parachute combination.

Of this varied assortment, the 5-inch spinner with high-capacity head and 5000-yard range was completed first and went into extensive Service use. The Institute produced a total of over 70,000 rounds for the Navy, filling the interval until the Bureau of Ordnance production came in.

The other spinners called for by the Navy program were in varying stages of development when the war ended. The completion of the series is included in the rocket program which the Navy took over as a part of its continuing peacetime research and development activity.

## Aircraft Spinners

Consideration of spinners for aircraft began while the development of the 5-inch HVSR and the barrage spinners was still under way. Interest in aircraft spinners probably originated because of a new development in German Air Force tactics. German jet planes armed with heavy nose guns were attacking our bombers astern, from outside the effective range of the 50-mm. tail guns. It was suggested that spinners retro-fired from the tail-gun positions of the bombers might be an effective countermeasure. Some preliminary work was done at Inyokern, to investigate the firing of small 2.25-inch spinners in clusters and 5-inch spinners in single shots. The dispersion in retro firing was found to be very high and the spinner behavior erratic, and no final development was completed.

In the meantime, however, a program had been started for the adaptation of spinners for forward firing from aircraft. The carrying out of this program was a long and complicated business, involving a more rigorous theoretical analysis of rocket behavior than had been made before, and necessitating many design modifications and correlated test firing. By the time the war ended, a forward-fired 5-inch spinner had been developed

which was at least as accurate as the best of the forward-fired fin-stabilized rockets.

There were obvious advantages in spinners for forward firing. The finned rocket, to have stable flight characteristics, must be long in proportion to its diameter (Holy Moses is 5 inches in diameter and 69 inches long. The barrage spinner is 5 inches in diameter and about 32 inches long.) The spinner is relatively short and stubby. This means economy of space in shipping and storage, and greater simplicity in handling because there are no fins which must be attached as part of the assembling process.

The launching problem, however, is more difficult. For aircraft firing, a tubular launcher is indicated, but to hang tubular launchers under the wings of a high-speed fighter plane would create an intolerable amount of drag and seriously impair the plane's maneuverability. Consequently, the launching tubes must be incorporated within the fuselage or wings. This, in turn, necessitates changes in the structural design. But if the launching tube could be put into the fuselage, then there would be the possibility of manual reloading, and the plane could carry a larger effective rocket load than is possible with the externally mounted fin-stabilized rockets.

The launcher problem was being attacked while the forward-fired spinner was under development. Considerable work was done on closed-breech launchers with provisions for blast effect and elimination of recoil, and several launcher designs were being engineered in collaboration with airplane manufacturers. The final development of satisfactory launchers for forward-fired spinners was another piece of unfinished business at the end of the war, to be completed by the Navy as a part of its continuing research and development program.

## Theoretical Analysis and the Solar Yaw Camera

Spinner development, as a whole, was probably the most complicated project which the California Institute group undertook. The difficulty can be stated somewhat over-simply by saying that the spinners, on the basis of preliminary theory, had an unfortunate habit of taking matters into their own hands and behaving contrary to prediction. In spinners, a much more complicated variety of forces was involved than in finned rockets. The problem, then, was to gain a clearer understanding of these forces and their interaction and then to apply this knowledge to design.

Part of this work devolved upon a group headed by Clarence Weinland, which was known among the Institute rocketeers as the Spinner Society.

They devised a variety of experimental launchers in an attempt to find out to what extent launching was responsible for the behavior of spinners. They also did a great deal of work with dynamically balanced rounds (that is, rounds in which the mechanical imbalances which are inevitable in

fabrication and assembly were eliminated). They also fired rounds with known amounts of imbalance, so that the effects of imbalance could be measured.

Along with this pragmatic activity, Davis's theoretical group worked on a more rigorous analysis of fundamental theory. The most important contribution here was made by J. W. Follin, Jr., who developed a much more rigorous and searching analysis of wind effects on spinners than had ever been made before. Extensive range firing, with careful wind measurements, bore out completely the predictions which could be made on the basis of this analysis. This cleared up one of the major spinner perplexities.

For theoretical analysis, a new instrument, developed especially for recording spinner behavior, provided more detailed and extensive data than had been hitherto available. Bowen's acceleration camera covered only the first part of rocket flight from the launcher, and this only for ground-fired rockets. Since the erratic behavior of spinners occurred beyond the range of this type of camera, what was wanted was a device that would record data for the full flight of the rocket, whether ground- or air-fired.

The answer was the solar yaw camera. The essential idea for it was Smythe's; Bowen assisted materially in the development. The solar yaw camera is a device mounted as the nose of a spinner. In essentials it is a pinhole camera, in which the rotational motion of the rocket moves a film strip continuously past the light-admitting aperture. It begins operating as soon as the rocket is fired. Each rotation produces one exposure on the film strip. The record, when the film is developed, appears as a series of vertical lines of varying height, spacing, and vertical displacement. Careful measurement of these record lines gives a complete record of the behavior of the rocket during its whole flight. Such data provide the theoretical analyst with checks on theory, and give him closer insights into the forces involved in rocket behavior, which, in turn, enable him to develop and refine fundamental theory.

By the time the war ended, then, the California Institute group had developed several spinners for Service use, had a comprehensive spinner program well under way, and had formulated a body of fundamental spinner theory which will be basic to any further spinner developments, no matter what Service or agency carries them on.

## Combat Use

The foregoing description of spinner development will suggest, as was the fact, that it took place too late in the war for much combat experience to develop. The 3.5-inch developed originally for the Marines was awaiting overseas shipment for the OSS when the war ended. Two 5-inch-caliber spinners did, however, see actual service.

5-inch HVSR's, crash-produced at the Institute, were dispatched to the Philippines in 1944; late in the fall of that year Paul E. Lloyd[3] of the Institute group went out as an adviser for installation and training procedures. Intended for use on PT's against Japanese coasting luggers, the PT's, when ready, could find few of these targets. They were diverted for the most part to long-range bombardment of shore installations, with results that were difficult to assess.

The 5-inch, 5000-yard barrage spinners on the other hand had a history which deserves more extensive narration. They served both at Iwo Jima and at Okinawa.

In the fall of 1944, the Navy drew up general specifications for a rocket gunboat which was to have the maximum possible fire power. Following standard Navy practice, the hull was the responsibility of the Bureau of Ships; the armament, of the Bureau of Ordnance. After a study of the requirements, the Bureau of Ships concluded that the LSM (Landing Ship, Medium) hull, with some modifications, would be suitable and would save time, since a new hull design would not have to be worked out. For the Bureau of Ordnance, the chief problem was working out the design and development of a continuously reloadable launcher, and continuously variable elevation and train by director control. Each of the new rocket gunboats which were designated LSM(R) was to mount ten of these launchers. June 1, 1945, was set as the target date when the first flotilla of LSM(R)'s was to be in readiness at Pearl Harbor.

In the meantime, the California Institute group undertook crash production of 5000-yard spinners. It will be remembered that the launcher section of the Institute group developed a 12-round gravity-feed automatic launcher for spinners, similar to the 4.5-inch barrage-rocket launcher which had already been put to very wide use. In addition to 124 of these spinner launchers which the Institute group produced, 2000 more were procured by the Bureau of Ordnance from Consolidated Steel, which carried through crash production at their Orange, Texas, plant.

For the gravity-feed automatic spinner launchers a general usefulness was foreseen comparable to that of the 4.5-inch BR launcher. But more particularly, they were needed in a hurry because operations were then in the planning stage in which the spinners could be used to advantage, and these operations were scheduled to take place before the LSM(R)'s would be ready. Hence, as a temporary substitute, work was rushed through on mounting gravity-feed launchers on the decks of standard LSM's, which were designated LSM(R) (Interim). But the tempo of operations in the Pacific was stepped up, and even these interim LSM's could not be readied

[3]Holder of a Doctor's degree in physics from the Institute, he was engaged in research in physics until he joined the rocket group, where his work was mainly with land and amphibious launchers.

in time for the action where they were needed. Therefore, a substitute had to be provided for the substitute.

In December 1944 a flotilla of eighteen LCI's was therefore equipped with gravity-feed launchers at Hunter's Point near San Francisco. Three members of the Institute Launcher Section went north to help in this rush job: Gould on launcher installations, Bascom on wiring, and Meneghelli on crew training.

The emergency for which this flotilla of LCI's was needed was the invasion of Iwo Jima, in which they were a part of Vice-Admiral Kelly Turner's command. Each LCI mounted six gravity-feed launchers. Nine of the ships had a total ammunition capacity of 280 rockets each; the other nine, 1700 rockets each. The Gunboat Support Group consisted originally of two six-ship LCI units and two six-ship LCS(L) units (the latter firing 4.5-inch fin-stabilized barrage rockets). On February 17, 1945, two days before the Iwo Jima D–Day, the first 5000-yard spinners were fired against the Japanese, in support of the operations of underwater demolition teams. On that day, nine of the twelve LCI's were knocked out, leaving only three operational on D–Day (February 19) and necessitating a greater spacing out of the LCS(L)'s than had been planned.

Besides this rocket fire in support of the landing, a Rocket Support Unit of LCI's on D–Day delivered neutralizing fire on the Motoyama area inland. They fired their total load of spinners, 9500 rounds, by early afternoon, and were then withdrawn from fire support and assigned to other duties. Altogether 12,000 rounds of 5-inch spinners were fired on D–Day.[4]

The Okinawa operation, which was next on the docket, was also too early for the ultimate LSM's with continuously reloadable launchers. But four of the interim LSM's, mounting gravity-feed launchers, were ready. In rocket fire power, they were a tremendous improvement over the LCI's. Each mounted 85 of the 12-round launchers and hence could fire a total salvo of 1020 of the 5-inch spinners in approximately one minute. R. E. Sears,[5] a member of the launcher group, went to San Diego when these LSM's put in there on their way to join the fleet, and helped with the reworking of some of the launchers which had failed to perform properly. In January 1945, he went on to Pearl Harbor for further launcher work and crew training, and went out for practice firing with the flotilla before it sailed for Okinawa.

Besides these four interim LSM's, the flotilla comprised eight LSM's fitted with rail launchers for firing 5-inch fin-stabilized aircraft rockets. Each had rails for a deck load of 480 rounds, which could be fired in a

[4]8000 rounds of 4.5-inch fin-stabilized BR's were also fired on D–Day and another 2000 rounds from D–Day plus one through D plus 17.

[5]An engineering student who had joined the Launcher Section earlier in 1944. He had spent some time as a consultant at the Orange, Texas, plant of Consolidated Steel while the crash production of launchers was getting under way.

single ripple salvo in about thirty seconds. The use of aircraft rockets for barrage was admittedly a stopgap; but with the need for great barrage range and fire power, and the fact that the production of spinners and associated launchers was not yet in sufficient volume to take care of the whole flotilla, it was necessary to use the best available substitute, which was the 5-inch aircraft rocket. These rockets had a range of about 4000 yards, when fired from shipboard launching rails, and a TNT filler about equal in amount to that of the 5-inch spinner.

This flotilla of 12 interim LSM(R)'s under the command of Commander D. L. Francis, U.S.N., first went into action on March 26, 1945, and continued in operation through June 15, 1945. During this period they fired a total of well over 30,000 rockets. It is believed that the Okinawa operations represent the most varied and extensive use of rocket gunboats during the war. The experience during this period of nearly three months showed the variety of functions which rocket gunboats can perform, and how their effectiveness, after initial landing operations, can be increased by close co-ordination with ground forces ashore. The Okinawa operation opened up new possibilities which might have been completely realized by the ultimate rocket LSM's, which were en route to the western Pacific when the war ended.

The following account of the activities of this flotilla is summarized from the report of the commanding officer. For brevity, the term "F-ships" is used to indicate LSM's firing 5-inch fin-stabilized rockets; "S-ships," to indicate LSM's firing 5-inch spinners.

On March 26, 1945, four S-ships made a rocket attack on the southern beaches of Aka Shima in the Kerama Retto group, also to neutralize enemy personnel during a landing.

On March 27, six F-ships fired rockets into the town of Arawe on Tokashiki Shima in the Kerama Retto, effecting a neutralization of enemy personnel in the town and its adjoining flanks which allowed landing troops to penetrate over 2000 yards before encountering even small-arms fire from the enemy.

On April 1, four S-ships and two F-ships made a rocket attack on the town of Takashippo on Okinawa Shima, firing 2900 rockets on enemy troop and mortar concentrations, securing indefinite neutralization until the beachhead was controlled. On the same day, six F-ships attacked the town of Guzukuma, southern Okinawa Shima. This attack effectively knocked out a primary road and a narrow-gauge railway which otherwise could have been used by the enemy to transport troops and equipment to the fighting front farther north.

Beginning on the night of April 13, a unit of two F-ships and four S-ships conducted night offensive patrols and harassing bombardment of Ie Shima. The rocket fire was extended through the entire night, at the

rate of 12 rounds per hour from each F-ship and 25 per hour from each S-ship. Each ship was given a previously designated target, but the fire was unco-ordinated in order to add to the enemy's confusion and secure the maximum harassing effect. On the morning of April 16 this unit fired 2600 rockets on enemy troop concentrations to the right flank of the landing beaches on Ie Shima, for neutralization or destruction of all exposed enemy personnel in the target area.

On the morning of April 19, one F-ship and four S-ships made a rocket attack on the southern Okinawa beaches to support a demonstration landing. The object again, in addition to destroying an enemy radio installation, was the neutralization of enemy troop concentrations.

From May 26 to 29, four F-ships worked in conjunction with Army Forces on Okinawa Shima. These ships were given definite targets by aircraft observation spotters provided by the Army. They made rocket attacks on enemy personnel, mortar concentrations, exposed material construction, etc. — targets assigned to them by the Army observation spotters. The work of the rocket ships was acknowledged by the Army command as being instrumental in enabling the eventual breakthrough of the Naha-Shuri-Yonabaru Japanese defense lines by our forces.

On June 3, four S-ships conducted a rocket attack against the island of Iheya Shima, firing 3000 rockets on the area directly behind the landing beaches and also on the flanking hills. The effect here was one of temporary neutralization.

On June 9, four S-ships conducted a rocket attack on Aguni Shima, firing 2850 rockets in support of troop landings, and also to neutralize enemy troops in this area until the beachhead was established.

From June 13 to 15, four S-ships were assigned to the 10th Army artillery section. These ships conducted both day and night bombardment fire on the southern tip of Okinawa against last-stand defenses of the Japanese forces on the island. This fire was conducted with the aid of aircraft observation spotters assigned by the 10th Army command. During this period, the rocket ships also used rapid fire on small troop concentrations which moved from one position to another, on enemy mortar concentrations, and on light, impermanent construction. By having direct communication with observation spotters, corrections could be made immediately and the desired effectiveness could be achieved before the enemy could ease his condition.

The troop command to which these four ships were attached reported that the effectiveness of the rocket fire was excellent at all times. In the opinion of the rocket command, the rapid gains made during the three-day rocket bombardment aided greatly in the eventual securing of this final objective by our troops on Okinawa.

The varied employment of these twelve interim LSM rocket gunboats

indicates the sort of job the ultimate rocket LSM's would probably have been called on to perform. Each was equipped with two 40-mm. and four 20-mm. mounts, four 4.2-inch chemical mortars, one 5/38-inch dual-purpose gun, and ten continuously reloadable rocket launchers for 5-inch spinners (the launchers having a firing rate of 30 rockets per minute, that is, 300 rockets per minute per ship). Thus each of the LSM's would have had a terrific fire power, and there is little doubt that they would have been effective in action.

The Navy's plans called for a total of 48 of these ultimate rocket LSM's. The first of them were finished and on their way across the Pacific when the war ended.

One other combat use of 5-inch spinners remains to be chronicled. This use, it must be confessed, probably did not make any very significant contribution to the winning of the war, but it is included here as a fine example of aggressiveness and enterprise. As word of the 5-inch spinner spread through the fleet, the submarine service developed considerable interest in mounting rocket launchers on submarines for nuisance and diversionary raids on the Chinese and Japanese coasts.

One submarine, the *Barb,* of the ships under Commander, Submarines, Pacific Fleet, was equipped with an automatic launcher for spin-stabilized rockets. In this installation, the base plate of the launcher was permanently attached topside of the submarine and permanent wiring was installed in a waterproof junction box. Rockets and launcher were stowed below. When the submarine surfaced at an appropriate distance from the target, the launcher was brought topside and attached to the deck frame and loaded, and a temporary cable connection made between the firing control and the junction box. This whole process, including the loading of 12 rockets in the launcher, could be completed in three minutes.

In three different attacks, this submarine fired a total of 72 rockets in shore bombardment of the island of Honshu, reporting that all the rounds fell within the target area. So, before the war ended, spinners were fired against one of the Japanese home islands.

# CHAPTER XXIV

# *THE WINDUP*

THE SPINNER series was the last major rocket development which the California Institute group undertook. By the close of 1944, although the Bastogne episode had dampened too facile optimism about a quick and easy end of the war in Europe, it was nevertheless clear that the end was at last in sight. No one could predict with certainty how long it would take to finish off Japan after victory in the European Theater, but the general estimates were such as to make OSRD take stock of its future policy. Since it was set up for the duration of the emergency and scheduled for liquidation as soon as possible after the war ended, the question was whether new developments undertaken under NDRC sponsorship could be completed in time to go into Service use against the enemy. If not, there was no justification for undertaking them.

## Kamikaze Countermeasures

The rocket work carried on by the Institute group under contract OEMsr-418 in 1945 therefore consisted only in the completion of developments begun earlier and a few new applications of rockets already developed.

Most of this work concerned countermeasures against Jap suicide bombers (kamikazes). The Japanese began developing the suicide-bomber attack as standard tactics against our naval vessels in the fall of 1944. A new type of weapon or attack always brings forth countermeasures; and, as might be expected, rockets figured among the countermeasures which were initiated.

The California Institute group was concerned with two different uses of rockets as countermeasures. The first was the use of cable-tied rockets. This development consisted of firing pairs of 5-inch aircraft rockets connected by a cable. The rockets were fired from launching rails attached to a standard gun mount which, by means of a self-contained motor drive, could rapidly change the elevation and direction of the rails. A special mechanism connected the cable with the fuze-detonators of the rockets, so that when the cable intercepted any obstacle — the wing or any part of the suicide plane — the high-explosive-loaded heads of the rockets would be set off. For test purposes, a single pair of rails was installed on the gun mount; for Service use, however, four pairs of rails could be used. Final

tests at Camp Pendleton demonstrated the feasibility of this equipment, but the war ended before it was tried out in Service.

The second application of rockets as countermeasures involved various uses of spin-stabilized rockets. Since it was desirable to use equipment which was already available, the first phase of the investigation consisted in finding out at how high an angle the automatic gravity-feed launcher could fire spinners. For barrage fire, 45 to 50 degrees elevation was ordinarily satisfactory; for firing rockets against suicide planes, which often attacked at very steep angles, a higher elevation was necessary.

The second phase of the investigation was concerned with special fuzing of spinners. Extensive firings were carried out using time-delay fuzes. These exploded the rockets at a predetermined distance from the launcher, producing a good pattern of air bursts. The most promising fuzing employed the proximity fuze, for the effectiveness of rockets with such a fuze did not depend on a direct hit or pattern of air bursts.

The third phase of the investigation, which was carried on concurrently with the fuzing experiments, consisted in launcher adaptation and development. Various combinations were worked out. For example, one developed by the Bureau of Ordnance consisted of a group of automatic launchers attached to a standard mount which was rapidly adjustable in elevation and lateral aim. The continuously reloadable launcher, which the Bureau had developed for the LSM(R), although not originated as a kamikaze countermeasure, was promising for this purpose because it was capable of high-angle fire, and its elevation and lateral aim could be changed by director control without interrupting firing.

Like the cable-tied rockets, however, the spinner countermeasures were not put into Service use before the end of the war.

## Project Groundhog

In the spring and summer of 1945, Tiny Tim, the 11.75-inch aircraft rocket carrying 150 pounds of TNT, was drafted as a special-purpose weapon. It was believed that it might be effective for ground firing against Japanese cave-installations. Experience on Iwo Jima and Okinawa had shown the need of such a weapon; and it was anticipated that in the attack on the Japanese home islands, similar cave installations would be encountered.

The problem of using Tiny Tim against caves was primarily one of devising launchers which could be quickly brought into position, and assembling and loading the rounds with only the limited facilities which would be available under field conditions. The launcher group developed two portable launchers for this purpose. One consisted of rails mounted on a two-wheeled trailer; the other, of rails mounted on a steel frame. The latter was demountable into two parts, each of which could be carried by

two men. Both launchers could be man-handled into position and emplaced for firing in a relatively short time.

A final demonstration was staged at Camp Pendleton in the summer of 1945, not long before V–J Day, with a Marine detachment emplacing the launchers and assembling and loading the rounds under an approximation of Service conditions. Some twenty-five Tiny Tims were fired at simulated Japanese caves, at ranges of 1000 and 700 yards, and several hits were scored. The end of the war came, however, before any decision was made as to whether this development was to go into Service use.

The kamikaze countermeasures and the launchers and techniques for ground firing Tiny Tim against caves were special developments called for by special necessities. The need for them ended with the end of the war. As for the other work which the Institute group was carrying on, plans had already been made for its eventual disposition, and V–J Day served only to accelerate the carrying out of those plans. Since practically all this work was for the Navy, transfer to the Naval Ordnance Test Station, Inyokern, made continuation possible, where this was desirable.

The transfer included current work on the improvement of the 5-inch HVAR and Tiny Tim, the completion of the spinner series, and further launcher development for aircraft rockets and spinners. Details of the transfer and disposal of physical facilities are discussed in the following chapter.

Three projects were so far along when the war ended that the Institute group finished them. The first was a firing program at Goldstone to complete the data for exterior ballistics of fin-stabilized rockets. The second was the automatic computing sight for aircraft-rocket firing, which has already been discussed in Chapter XXI. The third was the DDR fuze (Deceleration-Discriminating Rocket fuze) for fin-stabilized aircraft rockets.

It will be recalled that when the 5-inch aircraft rocket first went into operational use in the Pacific it was equipped with an instantaneous nose fuze, and that very soon Service reports stressed the need of delay fuzing to make aircraft-rocket fire more effective. Except when the targets were personnel and exposed positions, a delay was needed so that the rocket head could penetrate the target (e.g., shipping, bunkers, etc.) before detonation occurred. Since the development of delay fuzes had been undertaken some time before, this request could soon be met; and eventually the standard practice was to fuze the 5-inch rockets (and later the 5-inch HVAR's) with both instantaneous nose and base delay fuzes, the former being fired safe (that is, inoperative) while Tiny Tim was base-fuzed, since it would be fired only against targets requiring penetration before detonation. All of the delay fuzes which the fuze section of the Institute group developed were designed for insertion in the base, or aft end, of the rocket head. All were armed by a buildup of gas admitted from the rocket motor. Impact against

the target initiated the fuze action, one stage of which involved a mechanical or pyrotechnic element that caused a time delay between impact and detonation. Such fuzes could be designed, of course, for various delays; but once the duration of the delay had been decided on, it was built into the fuze, as it were, and could not then be modified. This was the type of delay fuze that was available (in several different models) for aircraft rockets. That it was adequate is demonstrated by the impressive record made by rocket-firing Navy and Marine Corps planes.

But like most other things, delay fuzes were capable of improvement. The nature of the desired improvement was indicated by frequent discussions of how long a delay a fuze should be designed for (the commonest, in practice, was .02 of a second). The difficulty was that for maximum destructive effect, different delay times were needed for different types of targets — palm-trunk bunkers, caves, reinforced concrete, armor plate, etc. It was not impossible to design a fuze in which the delay could be varied by changing a setting device, but the obvious complications of such a system would have made it impracticable for Service use.

The fuze group made a more fruitful approach to the problem based on the following facts. A rocket decelerates, of course, as soon as it strikes the target. The rate of deceleration is determined by the nature of the target material; the deceleration is more rapid, for instance, on high-strength concrete than on palm logs.

Why not use this variation in rate of deceleration, then, as the means of determining the time delay? This was the line which the fuze section followed — to develop a fuze in which the time delay would be a function of the rate of deceleration. Hence, the name: deceleration-discriminating or delay-discriminating fuze. The specific means which were used to translate this general principle into a working fuze cannot be disclosed. For the present all that can be said is that the development was successful, although too late for Service use. The DDR fuze was tested with satisfactory results shortly before the war ended, and the final design was finished, like the spinner firing and the aircraft-rocket sight, a few weeks after V–J Day.

With the completion of these projects, the research and development work of the California Institute group was ended.

## Unfinished Business

Thus, save for some mopping up, the California Institute group ended their work almost exactly four years after they had begun. In the work of those four years they had fulfilled their primary purpose of developing new weapons in time to get them into effective Service use. After their first few months of operation they carried on their research and development projects in increasingly close co-operation with the United States

Navy. They developed practically all the rocket weapons which the Navy put into Service use — the Mousetrap and the retro-fired bomb for antisubmarine warfare, the barrage rocket for amphibious operations, the series of forward-fired aircraft rockets, and finally, the spinners. One or another of these types of rocket ordnance went into action in practically every area of operations. For the Institute group this meant the satisfaction that they had helped to make up for the neglect of military research in the years before 1940, and the greater satisfaction that their work had resulted in weapons that helped to wage war more effectively, which meant, in the last analysis, to achieve victory with a smaller expenditure of American lives.

# Termination

## CHAPTER XXV

## *TERMINATION*

THE WEAPON developments and related studies carried out under the contracts with the Rocket Ordnance Division have been described; now in this final section the termination of the activities and the disposition of the facilities acquired will be outlined. As Cal. Tech. rocket activities began to fall off early in 1945 their termination will be described first.

1. Designs of all the Cal. Tech.-developed rockets that were destined to be used in Service were essentially complete by the end of 1944; from that time the continuing rocket activity was concerned mainly with the use of rockets on Navy and AAF planes, improvement of aircraft-rocket firing sights, studies of the ballistics of spin-stabilized rockets, new type fuzes, and installation of equipment and propellant operation at the China Lake Pilot Plant.

The decrease in rocket research was in accordance with the OSRD demobilization policy that the time for basic research leading to new weapons was past for this war, and the emphasis should shift to Service testing and consequent adaptations, which would more properly be a responsibility of the Army and Navy organizations than of the OSRD contractors.

To some degree, the efforts of many of the Cal. Tech. personnel were diverted to another project as described next, which had some effect on the final months of the rocket work under OSRD and its transfer to the Navy.

Soon after his return from Europe in the fall of 1944, Dr. C. C. Lauritsen, Director of Research on the California Institute of Technology contract, became engaged also in work related to the Manhattan Engineer District project. Before long, requests were made for increased participation of the Section L contract group on special phases of this project. This was authorized by the National Defense Research Committee at its meeting of December 15, 1944.[1]

Immediately the problem of division of effort between this highly

[1]The first authorization was for a relatively small sum, $25,000, but the work steadily increased, until in the end a total of $7,500,000 had been expended.

secret program and the rocket program became serious, as much more was involved than simply setting up a section to handle the special work as had been done in previous cases of interdivisional co-operation.

At this time, plans were already under discussion for the transfer of responsibility for rocket research and development to the Naval Ordnance Test Station, Inyokern, California; so it was felt that taking on the new work would not add to the over-all magnitude of operations under the California Institute of Technology contract but would hold the organization at a high level of activity longer. In general this proved to be the case, but naturally the Navy was loath to participate in action that might mean it would not continue to receive, as Admiral Holmes expressed, "the same high quality level in achievement."

A number of conferences took place in Pasadena during the winter of 1944–45, attended by National Defense Research Committee, Manhattan Engineer District, Bureau of Ordnance, and California Institute of Technology representatives, in which policies were adopted covering the participation of the Section L contract organization in this added activity. MED visitors included Dr. Oppenheimer, General Groves, General Farrell, and Captain (now Rear Admiral) Parsons. The work, although carried on through the existing Division 3 contract with Cal. Tech., was not under the concern of the Division 3 members. The technical direction was the responsibility of a special NDRC committee consisting of Conant, Hovde, and Hartley Rowe (Chief of Division 12). The Division 3 staff supervising the California Institute of Technology contract functioned for this project under the authority of this committee.

Although technical personnel on the contract could soon identify the project as part of the Manhattan District program — visiting scientists well known to be active in it were indication enough — in general the impression[2] seemed to exist that some super guided rocket was involved.

As mentioned above, plans were already being discussed for the transfer of the responsibility for rocket development to the Naval Ordnance Test Station, Inyokern. A committee representing the Station, Cal. Tech., and Division 3, NDRC, under the chairmanship of Captain E. S. Burroughs, Commanding Officer, discussed the transfer problems in the fall and winter months.

In April with NDRC approval, most of the 200 members of the Inyokern Range Operations Group (known as Section E of Contract OEMsr-418), under Dr. Ellis, accepted Navy Civil Service appointments. This group was transferred, not only for the purpose of operating the test ranges on

[2]There was considerable apprehension when, very soon after the collapse of Japan, a café across the street from a location at which some of the California Institute of Technology-Manhattan Engineer District work was centered put up a sign in neon lights, "Atomic Inn." Investigation by the counterespionage people, however, established this action as pure coincidence.

the Station, but also to serve as a nucleus for the growth of the Research and Development Department which was to be headed by Dr. L. T. E. Thompson.

Dr. W. A. Fowler, Assistant Director of Research of the Section L contract, spent much time and effort at Inyokern, prior to the arrival of Dr. Thompson, in the spring of 1945 in assisting the Navy in getting the Research and Development Department in operation. The Office of Scientific Research and Development under a special project authorized technical assistance, continued ammunition deliveries for experimental purposes, and limited emergency procurement of research equipment and supplies for several months in order to make the transfer with a minimum loss of effectiveness in prosecution of the rocket program. In turn the Navy provided personnel and facilities for the still active projects assigned through NDRC to California Institute of Technology.

In February 1945, definite proposals were made that the facilities of the Developmental Engineering Section of the contract be turned over to the Navy, which could then make a prime contract with an industrial concern to operate them, and in this way provide for crash procurement and Inyokern's experimental needs independently of the Office of Scientific Research and Development and California Institute of Technology.

For a number of reasons, however, definite action was not taken at this time — one point being the high value that the research groups placed on the setup which provided for large-scale experimental ammunition and equipment requirements without going outside the contract organization.

This section, after delivery of the 45,000 spinners in December, was called on to continue furnishing crash-production services for the spinner program well into the spring — providing over 200,000 motors and heads for the various types of rounds of the spinner family as well as Tiny Tim, for experimental purposes, both for Navy and for California Institute of Technology projects.

Early in the spring a large fraction of the Cal. Tech. spinner procurement program, including tooling and supplies, was transferred to the Bureau of Ordnance, Navy, which entered into a direct contract with Oedekerk and Ludwig Company, which had been an important Cal. Tech. subcontractor, in Pasadena to carry it on. A similar transfer involving the Navy and Oedekerk and Ludwig had been made in the late summer of 1944 in the case of SCAR (Subcaliber Aircraft Rockets) production which was badly needed for rocket training programs.

In April 1945, the Division 3 members recommended the transfer of the Section L contract activities to the Navy.

A conference was held in June in Pasadena attended by representatives of the Bureau of Ordnance, NOTS, Inyokern, Division 3, NDRC, and Cal. Tech., at which agreements were reached with regard to the future activi-

ties of OSRD contract OEMsr-418 and a program scheduled for its transfer and termination. At this meeting, Captain Entwistle, Director, Research and Development Division, Bureau of Ordnance, outlined the Navy plans. J. R. Page, President of the Cal. Tech. Board of Trustees, stated the position of the Institute, and F. L. Hovde summarized OSRD policy.

Captain Entwistle pointed out that plans for the transfer of rocket work to Navy supervision were well underway, but emphasized the need for a program of postwar fundamental research, fostered by the Navy, in which colleges and institutions should co-operate.

Mr. Page stated that the Board of Trustees was well aware that winning the war was still the paramount consideration, but it would be relieved, because of the financial magnitude, to have a transfer of the Contract 418 work effected so that a return could be made to the Institute's normal job of teaching and fundamental research.

Mr. Hovde said that OSRD was definitely a wartime agency and could not approve projects for the postwar period. Therefore, now that the Navy had set up an adequate organization to take over a large part of the work, transfer should be accomplished in the next six months.

Specifically the following plans were made:

a. China Lake Pilot Plant was to be transferred December 1, 1945, with Eaton Canyon closing down by October 1, but remaining in a stand-by status.

b. A target date of September 1 was named for transfer of operations at Morris Dam, which could continue as a substation of Inyokern.

c. Section B organization, including one third of the plant on Foothill Boulevard and some other city facilities, would be transferred by September 1, with the Navy arranging by prime contract with the General Tire and Rubber Company of California for operation.

d. All other operations would be terminated by December 31, 1945, except for editorial work on results of the contract program, which should be continued as long as necessary to complete the job properly.

The transfer of Section B operations was actually made on July 31, except for the real estate involved, which required additional time. Section B property (equipment, materials, etc.) was transferred to the Bureau of Ordnance, which contracted with the General Tire and Rubber Company of California for further operations. The transfer of the Section B operations without disruption called for careful planning in keeping track of inventories of materials in process and control of purchase orders and subcontract operations. This was in charge of Remington Stone, C. P. A., under the supervision of a property-transfer committee representing the Navy, General Tire and Rubber Company of California, NDRC, and Cal. Tech. Facilities and materials involved in this transfer represented expenditures under the contract of $3,117,000. Personnel transferred at the same time,

Trevor Gardner, Supervisor of Section B, becoming Vice-President and General Manager of the General Tire and Rubber Company of California.

The end of the war did not change the schedule for the other transfers materially. The experience gained with Section B was applied and all the rest were substantially completed by October 31, except for transfer of title in the case of real estate. The additional transfers to the Services covering the Pilot Plant at Inyokern, the rest of the Foothill plant, Morris Dam, Eaton Canyon, and property on the ranges at Goldstone, Pendleton, and Muroc (AAF), involved property costing about $5,000,000. Eaton Canyon equipment was transferred to China Lake and Picatinny Arsenal, with a part of the establishment left for continuation of a Division 8, NDRC, program on propellant research that BuOrd had arranged by Navy contract with the Institute. Dr. Sage after the transfer of the China Lake Pilot Plant continued to devote part of his time to Inyokern, not only to ensure that the presses and processing lines were operating satisfactorily, but also to help get the interior ballistics group well established.[3]

In accordance with termination schedules set up by OSRD after the surrender of Japan, research activities centered on the campus, scheduled to wind up in December according to the June plan, were practically at an end by September 30.

Formally the date October 31, 1945 was set for the completion of subject work, except for report preparation, under the contract. There was relatively little property left to be disposed of through the War Assets Corporation.

2. The subject work of the still active Section H contracts, except for reporting, was terminated rapidly during the final months of 1945 as required by OSRD demobilization plans, with the Services providing for continuation of some of the work[4] in case of the George Washington University, University of Minnesota, and Budd Wheel Company contracts, as described below.

In August 1945, the Chiefs of Divisions 3 and 8, NDRC, submitted a joint recommendation to the War Department Liaison Officer for NDRC in which the following points were presented with particular reference to the facilities available at the Allegany Ballistics Laboratory:

"(a) That the Services continue a co-ordinated program of research, development, and application of thrust and jet-propulsion units using solid fuels.

"(b) That the development program be carried out by a competent in-

[3]Sage's assistance to Inyokern continued through the summer of 1946.

[4]One important ABL activity during 1945 was work on a slow-burning thrust unit, designated "bumblebee rocket motor," developed for the Applied Physics Laboratory of the Johns Hopkins University operating under Navy contract.

dustrial contractor with centralized facilities for metal parts, engineering, powder, and testing.

"(c) That the availability to the War Department of NDRC facilities be pointed out to this contractor and that new facilities not be created unless the contractor can present arguments which we have not considered.

"(d) That action be taken as rapidly as possible so that, if it is desired by the contractor, a nucleus of experienced scientists may be available to continue this program."

This matter was referred to the Ordnance Department for the attention of the Chief of Research and Development Service. The War Department, however, indicated it did not have plans that would involve using the Allegany Ballistics Laboratory facility (much of the equipment, however, was requested for use elsewhere). Since one of the main activities in the closing months of the war was on a Navy project on launching of guided missiles, the interest of the Navy was investigated. Among the decisions reached at a conference on research and development of large thrust units held at the Bureau of Ordnance on the 21st of September was one that the Navy would indicate to NDRC its interest in the continuation of this type of work, after exploring the possibility of du Pont or other potential contractors carrying on the program at Allegany Ballistics Laboratory.

Represented at the conference, which was under the Chairmanship of Commander Hindman (Ret.), were Divisions 3 and 8 of NDRC, ABL, the Navy Department's Office of Research and Inventions, Bureau of Ordnance, Bureau of Aeronautics, Office Chief of Ordnance, Rocket Developments Division of the Army, and the Applied Physics Laboratory.

On September 25, Admiral Hussey, Chief, Bureau of Ordnance, formally advised NDRC that it had a definite need for the ABL facilities. Arrangements were made later whereby Hercules would operate the plant.

Certain equipment used in the continuance of development work carried on at ABL for the CWS was moved to Dugway, Utah. War Department property at ABL was transferred to the Navy without reimbursement by arrangements made through War Department Liaison Officer for the NDRC.

The transfer was formally completed on December 31, 1945.

The Navy made direct arrangements with the University of Minnesota for the continuation of work on propellants, while the CWS extended the operations carried on for Section H under its contract with the Budd Wheel Company.

Thus we find at the end of the war that the Services made plans to carry on at a basic level, with many of the same personnel and facilities, the rocket-research and -development program initiated at the beginning of the war by Section H, and carried through to significant results by the Division 3 contract groups.

# APPENDIX 1A

# *PERSONNEL ALLEGANY BALLISTICS LABORATORY*

(The numbers in parentheses refer to the August 1945 organization chart on pages 46–47.)

*Office of Director of Research (1)*
R. E. Gibson — Director
A. Kossiakoff — Deputy Director
M. H. La Joy — Research Associate
*Executive Office*
T. J. McCormick — Executive Officer
T. B. Calhoun — Research Associate
*Assistant to the Director*
A. Africano
J. Shryock — Research Associate
B. Branche
*Project Co-ordinator*
J. Burns
*Powder Operations (2)*
T. J. McCormick — Chief
T. Calhoun — Assistant Chief
*Powder Service*
G. Padgett — Supervisor
*Loading Operations*
D. Dembrow — Supervisor
*Powder Machining and Igniter Service*
F. P. Kelly — Supervisor
*Rocket Weapons Division (3)*
A. Kossiakoff — Chief
D. W. Osborne — Section Chief
J. B. Rosser — " "
J. Beek — " "
D. M. Brasted — Group Supervisor
M. Goldman — " "
H. C. Thompson — Chief Flight Range Foreman
S. Bernstein — Research Associate
M. R. Goff — " "
C. Blackson — " "
R. R. Newton — " "
T. L. Gallo — " "
E. D. Lewis — " "
S. Shulman — " "
R. Preston — " "
W. A. Hines — " "
G. W. Engstrom — " "
J. F. Kincaid — " "
S. Sherman — " "
M. Dresher — " "
G. F. Rose — " "
N. Marmer — " "
E. M. Cook — " "
W. J. Harrington — " "
G. L. Gross — " "
J. A. Brittain — " "
B. Grisamore — " "
M. B. Dwall — " "
R. J. Thompson — " "
E. A. Cook — " "
E. Debutts — " "
L. Streff — " "
R. Hayes — " "
D. Crowell — Research Assistant
P. Owens — Technical Assistant
M. Footen — " "
B. W. Kirby — " "
V. L. Haus — " "
F. McClure was a former Section Chief in this division.
*Engineering (4)*
A. S. Glenn — Chief Engineer
A. A. Fisher — Project Engineer
M. A. Johnston — Project Engineer

P. M. Freeman — Supervisor Design Engineering Department
M. Malcolm — Department Supervisor
C. Taylor — Supervisor Planning Department
R. I. Beddoe — Supervisor Production Control Department
E. K. Long — Foreman Drafting Section
G. D. Brewer — Research Associate
R. M. Hubbard — " "
A. L. James — " "
D. D. Grimes — Methods Engineer
H. C. Stumpf — Metallurgist

*Instrument Division (5)*

N. E. Alexander — Chief
*C. Lathrop — Consultant
H. Stern — Research Associate
W. Barber — " "

*Propellant Division (6)*

W. H. Avery — Chief
H. Higbie — Assistant Chief
L. D. Sachs — Executive Officer
R. S. Craig — Range Supervisor
C. A. Boyd — " "
S. S. Penner — " "
R. E. Hunt — " "
Lt. (j.g.) J. J. Donovan — Section Supervisor Themochemical
R. L. Arnett —Supervisor Propellant Production
W.A.Hendricks — Research Associate
J. F. Johnson — " "
S. D. Brandwein — " "
R. J. Taylor — " "
R. H. Bond — " "
D. Leenov — " "
R. Whiteman — " "
R. Evans — " "
N. Marans — " "
H. Fritz — " "
A. Bernstein — " "
T. T. Omori — " "
A. Turk — " "
A. J. Madden — " "
J. E. Sherwood — " "
C. F. Bjork — " "
M. N. Donin — " "
L. Gonyea — " "
A. Stefcik — " "
Dr. M. Allen — " "

*Recoilless Weapons Division (7)*

C. N. Hickman — Chief
Major A. R. T. Denues — Deputy Chief
R. B. Kershner — Assistant Chief
S. Golden — Section Supervisor
O. R. Roderick — Section Superintendent
G. Bowen — Assistant Section Supervisor
L. Morey — Assistant Section Supervisor
C. E. Curtiss — Research Associate
E. J. Moore — " "
J. M. Woods — " "
C. G. White — " "
W. P. Spaulding — " "
N. T. Grisamore — " "
J. Levin — " "
D. R. Thomas — " "

*Project Co-ordinator (8)*

J. Burns — Co-ordinator
W. A. D. Millson — Editor in Chief
I. Ives — Assistant
M. Walker — Chief Photographic Section
Z. Pressman — Chief Photographer
F. Peters — Librarian
M. Blash — Supervisor Stenographic Pool
S. Brown — Executive Officer
S. Sheel — Research Associate
H. Osgard — " "
L. Greiff — " "
J. Bishop — " "
H. Tucker — " "
B. Weissman — " "
M. Koritz — " "

* C. Lathrop formerly was Chief of the Instrument Division.

APPENDIX 1B

# PERSONNEL OTHER SECTION H CONTRACTORS

OEMsr-256 — Western Electric Co.

R. L. Jones — Bell Telephone Laboratory Technical Representative
D. A. Quarles — " "
H. O. Siegmund — Project Engineer
A. K. Bear
M. E. Jaeger
F. L. McNair

R. F. Mallina — Mechanical Engineer
M. E. Studney
L. Birzis
E. Graf
H. Hansen
J. Dietz
J. Mogler
W. H. Schweyher
C. A. Hasslacher
J. M. Melick
F. Reck
C. F. Spahn

B. F. Runyon — Electro-Mechanical Engineer
J. R. Irwin
C. E. Nelson

D. D. Miller — Electro-Mechanical Engineer
G. B. Baker
H. B. Brown
P. E. Buch
J. A. Burwell
J. S. Garvin
T. H. Guettich
P. T. Higgins
C. W. McWilliams
W. W. Seibert
F. A. Zupa
G. J. Brown
V. Crisinel
S. Dalane
H. C. Fahrbach
J. Kaplowitz
R. W. Mortimer
C. L. Prentiss
J. J. Quinn
W. Schneck

A. J. Synder — Contract Supervisor
S. R. Avella
W. B. Bigger, Jr.

J. R. Townsend — Engineering Associate
E. L. Fisher — " "
G. R. Gohn — " "
F. W. Goss — " "
J. P. Guerard — " "
G. J. Herbert — " "
I. L. Hopkins — " "
W. E. Ingerson — " "
R. C. Platow — " "
J. A. Waddell — " "
I. V. Williams — " "
C. S. Christensen — " "
C. H. Hitchcock — " "
P. P. Koliss — " "
W. A. Krueger — " "
C. A. Lovell — " "
K. S. Dunlap — " "
E. R. Morton — " "
E. W. Olcott — " "

H. M. Stoller — " "
O. H. Williford — " "

OEMsr-337 — 416 — 520 Hercules Powder Company
Henry N. Marsh — Manager of Smokeless Powder Operations
Robert M. Cairns — Director of Hercules Experimental Staff
Harvey B. Alexander — Superintendent of Smokeless Powder Development at Kenvil
A. M. Ball — Technical Director of the Hercules Operations at Radford Ordnance Works

OEMsr-702 — California Institute of Technology
Linus Pauling — Official Investigator and Director
Robert B. Corey — Assistant Director
A. O. Decker
H. Levy

OEMsr-716 — University of Minnesota
Bryce L. Crawford — Official Investigator and Director
Clayton C. Huggett

OEMsr-762 — University of Wisconsin
Farrington Daniels — Official Investigator and Director
Robert E. Wilfong
Norris F. Hall

OEMsr-733 — Duke University
Lyman Bonner — Official Investigator and Director
Marcus E. Hobbs — Associate Director

OEMsr-968 — Budd Wheel Company
C. L. Eksergian — Chief Engineer
W. W. Farr — Project Engineer
N. R. Droulard
D. Dyer
W. Bortman
E. Morrison
A. Smith
W. B. Pope
F. Haubner
F. Rider
T. Musselman
M. Plant
T. Barden
J. Marsh
W. Ruska
C. Stedman
G. Callebaut

APPENDIX 1C

# PERSONNEL CALIFORNIA INSTITUTE OF TECHNOLOGY

(The numbers in parentheses refer to the organization chart on pages 138–139. Scientist and supervisory personnel are listed (Winter 1944–45.)

*Section I — Design and Development*
W. A. Fowler — Supervisor

*Projectile Group (1)*
T. Lauritsen — Supervisor
C. W. Snyder — Ass't Group Supervisor
C. E. Weinland — " "
C. S. Cox — Research Staff
M. B. Gentry — " "
M. I. Lebow — " "
J. W. McConnell — " "
G. G. Mosteller — " "
R. H. Neil — " "
P. H. Taylor — " "
A. Keast — Research Assistant
H. M. Greene — " "
L. H. Mahony — " "
J. N. McClelland — " "
R. R. Hargrove — " "

*Interdepartmental Order Group (2)*
M. L. Green — Supervisor
H. T. Walstaff — Research Staff

*Fuze Group (3)*
R. B. King — Supervisor
D. E. Brink — Research Staff
H. E. Fracker — " "
J. B. Hatcher — " "
D. L. Kraus — " "
R. E. Martin — " "
J. W. Petty — " "
V. K. Rasmussen — " "

*Land and Amphibious Launcher Group (4)*
L. A. Richards — Supervisor
J. D. Bascom — Research Staff
P. E. Lloyd — " "
H. Meneghelli— " "
R. E. Sears — " "
E. C. Walker — " "
T. Lofgren — Technical Assistant
R. D. Ridgway — " "

*Special Launcher Group (5)*
A. S. Gould — Supervisor

*Ballistics Research Group (6)*
F. E. Roach — Supervisor
C. T. Elvey — Associate Group Supervisor
B. N. Locanthi — Research Staff
D. D. Locanthi — " "
N. U. Mayall — " "
J. N. Schmidt — " "
C. D. Swanson — " "

*Theoretical Research Group (7)*
L. Davis, Jr. — Supervisor
L. Blitzer — Assistant Group Supervisor
J. W. Follin — Research Staff
T. H. Pi — " "
P. W. Stoner — " "
J. G. Waugh — " "
L. I. Epstein — Research Assistant
W. D. Hayes — " "
Hsue-Shen Tsien — " "

*Drafting Staff (8)*
V. F. Ehrgott — Superintendent

*Mechanical Staff (9)*
S. A. Macallister — Superintendent

*Section II — Aircraft and Ballistics (10)*

C. D. Anderson — Supervisor
A. L. Melzian — Assistant Supervisor
C. H. Wilts — " "
P. E. Edelman — Research Staff
J. L. Kavanau — " "
F. W. Thiele — " "
R. B. Leighton — " "
E. C. Briggs — " "
G. M. Safonov — " "
G. Kendall — Design Draftsman
R. C. Poolman — Technical Aide
L. H. Gutterson — " "

*Fire Control Group (11)*

H. W. Babcock — Supervisor
J. I. Bujes — Research Staff
O. D. Frampton — " "
J. L. Fuller — " "
A. M. Shapiro — " "
R. E. White — " "
C. E. Weaver — " "

*Section III — Photographic Measurements and Exterior Ballistics (12)*

I. S. Bowen — Supervisor
A. H. Andrew — Research Staff
D. P. Barrett — " "
C. C. Baum — " "
F. Crandell — " "
E. Floyd — " "
J. B. Irwin — " "
J. J. Johnson — " "
J. Titus — " "
L. W. Reeder — Research Assistant
G. M. Reger — " "
L. Ware — " "

*Section IV — Underwater Properties of Projectiles (13)*

M. Mason — Section Supervisor
L. B. Slichter — " "
B. Hill — Group Supervisor
B. H. Rule — " "
N. Haskell — Research Staff
W. P. Huntley — " "
P. M. Hurley — " "
O. C. Johnson — " "
H. G. Taylor — " "
L. Abrams — Research Assistant
A. V. Bunker — " "
P. Y. Chow — " "
J. S. Fassero — " "
R. Fleisher — " "
B. Gale — " "
R. Harrington — " "
R. C. Jackson — " "
G. A. Spassky — " "
J. G. Wendel — " "
M. Zimmerman — " "
F. Frederick — Mechanical Designer
J. H. Hutti — " "
C. A. Mattson — " "

*Section V — Propellants and Interior Ballistics*

B. H. Sage — Section Supervisor
D. S. Clark — " "
W. N. Lacey — " "

*Research and Development (14)*

P. A. Longwell — Research Supervisor
J. H. Sturdivant — " "
R. N. Wimpress — " "
C. Allen — Research Staff
R. Bersom — " "
A. Billmeyer — " "
D. Botkin — " "
T. Bright — " "
W. Colburn — " "
W. Corcoran — " "
H. Eisner — " "
Q. Elliott — " "
H. Ferris — " "
H. Frantz — " "
W. Hansen — " "
A. Hopmans — " "
D. Lemair — " "
R. Olds — " "
H. Vinock — " "
A. Williams — " "
S. Altschuler — Research Assistant
M. Blatt — " "
J. Brown — " "

G. Gordon — " "
L. Green — " "
K. Korpi — " "
B. Levedahl — " "
E. Mead — " "
E. Miller — " "
J. Miller — " "

*Technical Control (15)*
J. I. Gates — Group Supervisor

*Operations (16)*
C. L. Horine — Group Supervisor

*Experimental Production and Shipping (17)*
E. T. Price, Jr. — Group Supervisor
L. J. Pollard — " "

*Inspection (18)*
I. Beadle — Group Supervisor

*Static Firing (19)*
J. H. Sturdivant — Group Supervisor
H. A. Baird — Group Supervisor

*Magazine (20)*
J. Bancroft — Group Supervisor

*Service Operations (21)*
Safety —
A. D. Ayers — Group Supervisor

*Engineering*
P. Sabin — Group Supervisor
R. Alcock — " "
F. Eaton — " "
R. Gorschalki — " "
O. Graybeal — " "
E. Olson — " "

*Installation and Maintenance*
J. Meskell — Group Supervisor

*Inventory Records*
A. L. Carleton — Group Supervisor
D. Taggart — " "

*Material Control and Stores*
H. Bree — Group Supervisor

*Payroll*
H. Meredith — Group Supervisor

*Timekeeping*
J. N. Walker — Group Supervisor

*Service Group*
A. Ford — Group Supervisor

*Accounting*
R. Bretzius — Group Supervisor

*Accounts Payable*
L. Ainsworth — Group Supervisor

*Procurement*
W. Grundy — Group Supervisor

*Shop Procurement*
K. Robinson — Group Supervisor

*Personnel*
A. D. Ayers — Group Supervisor

*First Aid*
F. M. Bogan, M. D. — Group Supervisor

*Reports and Editorial*
S. Bradshaw — Group Supervisor
D. McAllister — Group Supervisor

*Section V — (NOTS Inyokern) (22)*

*Operations*
R. C. Stone — Group Supervisor

*Technical Control*
L. S. Sinclair, Jr. — Group Supervisor

*Experimental Production and Control*
E. T. Price — Group Supervisor

*Inspection*
I. Beadle — Group Supervisor

*Safety*
A. D. Ayers — Group Supervisor
D. L. Dewing — Group Supervisor

*Personnel*
A. D. Ayers — Group Supervisor
D. L. Dewing — Group Supervisor

*Payroll and Accounting*
H. T. Jones — Group Supervisor

*Material Control and Stores*
H. Bree — Group Supervisor
H. T. Jones — Group Supervisor

*Engineering*
E. P. Burke — Group Supervisor
J. W. Wahler — Group Supervisor

*Maintenance*
F. Wood — Group Supervisor

*Section VII — Torpedo Launching*
F. C. Lindvall — Section Supervisor

*Research (23)*
R. W. Ager — Research
H. N. Bane — "
J. E. Carr — "

W. H. Christie — "
E. D. Cornelison — "
G. Downs — "
D. E. Hudson — "
A. S. King — "
R. Skeeters — "
R. B. Moran, Jr. — "
R. R. Stokes — "
F. R. Watson — "
J. H. Wayland — "
O. C. Wilson — "
U. E. Younger — "

*Engineering (24)*
S. Baker — Engineer
J. Bowen — "
T. Curtis — "
A. Ekman — "
J. French — "
R. W. Haussler — "
R. L. Janes — "
J. H. Jennison — "
D. A. Kunz — "
W. Lemm — "
O. Terrell — "
W. Saylor — "

*Section VIII — Special Ballistics (25)*
W. R. Smythe — Supervisor
W. B. Dayton — Research Staff
C. F. Robinson — Research Staff

*Section A Project Personnel*

V. E. Wilson — Supervisor

*Applicant Department (26)*
D. M. Slaybaugh—Group Supervisor

*Employee Clearance and Records Department (27)*
I. J. Miller — Group Supervisor

*Medical Department (28)*
F. M. Bogan, M. D. — Group Supervisor
A. E. Martin, M. D. — Group Supervisor

*Selective Service Department (29)*
T. W. Nobles — Group Supervisor

*Inyokern Department (30)*
R. W. Seibert — Group Supervisor

*Section B — Developmental Engineering Department (31)*
T. Gardner — Supervisor
E. P. Hollywood — Assistant to Section Supv.
L. M. Kiplinger — "

*Production Services Division (32)*
Purchasing, Priorities, Material Control —
A. E. Acker — Group Supervisor
D. S. Hammack — Priorities Officer

*Contract Service and Transportation (35)*
E. E. Tuttle — Group Supervisor
W. K. Tuller, Jr. — Asst. to Group Supv.

*Contract Service (36)*
J. T. Hoffman — Contract Co-ordinator

*Transportation, Shipping and Receiving (37)*
V. C. Jones — Manager

*Production Division (38)*
Stanley Guelson — Production Manager
R. T. Stevens — Chief Engineer
J. Trigg — Plant Superintendent

*Production Control (39)*
P. Clark — Group Supervisor

*Salvage and Machine Shop (40)*
W. J. McNally — Group Supervisor

*Pilot Production (41)*
B. Johnson — Group Supervisor

*Stenographic (42)*
D. E. Harris — Group Supervisor

*Section C — Field and Research Operations (43)*
W. N. Arnquist — Supervisor
F. W. Pierce — Assistant Supervisor

*Range Supervisors*
R. H. Cox — Goldstone
D. R. Procter — Muroc
H. L. Prindiville — Camp Pendleton
R. Bogart — Waverly Drive (Pasadena depot for ranges)

*Section D — Project Comptroller (44)*
H. Ewart — Supervisor
R. J. Abshire — Assistant Supervisor
*Accounts Payable Department (45)*
J. D. Pirie — Group Supervisor
*Auditing and Accountability Dept. (46)*
G. C. Kelsch — Group Supervisor
*Cashier and Government Accounting Dept. (47)*
C. K. Parks — Group Supervisor
*Cost Department (48)*
P. Green — Group Supervisor
*Payroll Department (49)*
R. A. Felnagle — Group Supervisor

*Section E — Inyokern Range Operations (50)*
E. L. Ellis — Supervisor
G. E. Kron — Assistant Supervisor
P. A. Agnew — Assistant Supervisor

*Research Group (51)*
G. E. Kron — Group Supervisor
B. O. Davis — Test Co-ordinator
A. H. Ramsay — Staff
L. Stellman — Staff
A. L. Sorem — Safety Co-ordinator
C. A. Wirtanen — Group Supervisor, External Ballistics
J. E. Thomas — Group Supervisor Terminal Ballistics
R. V. Adams — Group Supervisor, Aircraft Ballistics

*Section F — Construction and Maintenance (52)*
W. Hertenstein — Supervisor

*Section R — Editorial Staff (53)*
J. Foladare — Supervisor
R. L. Eby — Research Staff
E. L. Wheatfill — Research Staff
R. Winger — Research Staff

# APPENDIX 2

## *SUBCONTRACTORS UNDER OEMsr-273*

Contractors handled much of their procurement and fabrication requirements through subcontracts.

In the event any of these involved research and development, special OSRD approval was needed in order to ensure protecting, for one thing, the Government's interest with respect to patents.

The subcontracts of this type were approved as to form by the Contracting Officer of OSRD and as to substance by the Division Chief.

George Washington University had the following subcontracts involving research and development:

| Subcontractors | Subject Work |
|---|---|
| Carnegie Institute of Technology | Investigation on the physical and mechanical properties of propellants. |
| Catalyst Research Corp., Pittsburgh, Pa. | Igniter Research. |
| Duke University[1] | Ordnance Devices & Materials (Physical properties of propellants). |
| Thomas Gibbs & Co., Delevan, Wisconsin (Div. of George W. Borg Corp.) | Driver Rocket Design. |
| A. F. Holden, New Haven | Installation and operating of furnaces & equipment, experimental heat treatment facilities. |
| Metaplast Co., New York City | Development of cold-setting material for securing adhesion of materials containing nitrocellulose to metals. |

[1]For the propellant panel.

# APPENDIX 3

## *PROGRESS IN BASIC SOLID-FUEL ROCKET RESEARCH*

The Section H program described in the earlier chapters carried through the summer of 1945 at a high level of activity. In addition to these developments, a report submitted by Dr. Gibson on the status of ABL projects as of V–J Day describes some of the fundamental work that was going on. In order to give the reader a view of some of this important work, parts of the report are summarized here.

For example, the theoretical groups had in preparation reports on the fundamental studies in gas-flow theory applied to the interior ballistics of rockets, on generalized theoretical treatments on exterior ballistics, on advanced theoretical studies of the motion during burning of fin- and spin-stabilized rockets, and on the motion of towing rockets, used in drawing out lengths of rope, cable, hose, or similar flexible material.

Since there was Service interest in lightweight, high-impulse rocket motors, various materials, including a heat-treated aluminum alloy tubing and a new laminated glass fabric, were being investigated.

Investigations at ABL of the burning properties of propellants are described briefly in the report, and the next paragraphs outline these studies:

"When this work was initiated, only fragmentary data were available regarding the burning properties of rocket propellants. It was known, however, that all existing rocket propellants gave pressures so dependent on the initial powder temperature that the useful temperature range in which the rockets could be fired was narrower than that encountered under service conditions. At low temperatures the powder burned in an unstable fashion; at high temperatures excessive pressures were developed.

"Studies Leading to Improvements in Rocket Propellants.[1] The initial stages of the investigation were concerned with the development of accurate methods of measuring the dependence of the burning rate upon the pressure and temperature. It was found that this dependence, in the Service temperature range, can be approximated closely by the formula

$$r = \frac{cP^n}{T_{1\text{-}T}}$$

where r is the burning rate (inches per second), T, the powder temperature, and n, c, and $T_1$ are constants for a given powder composition.

[1]The work of the Propellant Panel established by the joint Army-Navy Committee on New Weapons and Equipment is summarized in Appendix 4.

"On the basis of this formula, a study was made of the effect of changes in composition on the burning properties of an extensive series of double-base powders as the basis for determining the effect of different powder formulations upon performance. Significant results of this work are summarized as follows:

"1. The improvement in low-temperature performance produced by the incorporation of potassium salts in powders was discovered. This resulted in the adoption of an improved propellant for the Army 4.5-inch rocket, with a significantly better temperature range.

"2. The shape of the pressure-time curve for a powder burning in a rocket chamber was shown to be greatly affected by the amount of radiation absorbed by the powder at the burning surface and in the interior of the web. This has provided the basis for the proposed replacement of diphenylamine by carbon black and ethyl centralite in Army rocket propellants.

"3. The dependence of the burning rate of a powder and of the temperature coefficient of the rate on the heat of explosion was established. With this information the burning properties of powders may be predicted from the composition. A direct result has been the development of slow-burning powders, which make possible the design of high-performance rockets having a temperature range meeting all Service requirements.

"The request from the Services for the development of a smokeless JATO unit led to the investigation of propellants cooled by the substitution of appreciable amounts of nitroglycerin and by plasticizers, particularly triacetin. These propellants have been found to display remarkably good ballistic properties if burned in the optimum pressure range."

* * * * *

"Measurement of Burning Rates. Several methods have been employed for the determination of burning rates. The major experiments have used vented vessels specially designed to yield information not seriously affected by port area effects. Strand rates and rates in closed bombs, obtained on selected powders, have permitted estimates of radiation and erosion effects by comparison with the results obtained using the same powders in the vented vessel.

"Prediction of Interior Ballistics Behavior. Comparison of the burning properties of powders in experimental and Service weapons led to an investigation of the factors dependent on motor design which influence the internal ballistics of rockets. The shape of the pressure-time curve produced by a burning powder is influenced by the amount of radiation striking the powder surface, as well as by erosion, an effect earlier reported by British workers, i.e., the velocity of the powder gases streaming past the surface of the powder toward the nozzle.

"To obtain quantitative information regarding radiation, apparatus was constructed to measure the flame temperature of the powder gases, the emissivities, and the absorption spectra of powder of different compositions. From these studies was developed a theory of internal ballistics by which the shape of the pressure-time curve for a given powder and rocket chamber may be estimated with fair accuracy. The erosion effect has been studied for a variety of propellants with the use of the partial burning technique."

Also developments of materials and techniques for the restriction of burning on certain surfaces of powder grains were given.

A thermochemical group worked out methods for the accurate photometric determination of carbon black, volumetric determination of potassium sulphate by titration with barium chloride using tetrahydroxyquinone as indicator, and adaptation of the Kjeldahl technique for determination of nitroglycerin.

Calorimetric work was directed towards providing information for the correlation of burning rates and heats of explosion of propellent powders.

Extrusion research was conducted in order to ascertain the conditions necessary to produce solventless powder with satisfactory physical characteristics in granulations required for various designs of rockets. The physical testing of propellants required the design and construction of suitable apparatus.

Four processes for making grains were investigated. These were (a) solvent extrusion, (b) solventless extrusion, (c) casting, (d) pressure molding, each with its own advantages and limitations.

We have seen that solvent-extruded grains do not "dry out"[2] properly to permit using a web thickness greater than about one-half inch. On the other hand, powder granulated by this process is tougher and harder than that of the same composition obtained by other methods, and is useful where thin-web grains strong enough for rapidly accelerated rockets are desired.

The solventless-extrusion process became the most important source of rocket grains during the war. It has certain advantages in that the grains extruded are not limited in web thickness, and exact control of shape and size (where lengths are large compared to diameters) is readily feasible. To control the hazards[3] involved, however, not only in extrusion itself but in the preliminary hot rolling[4] and subsequent machining operations heavy machinery and barricades and other elaborate safety precautions are required.

The casting process offers many advantages but is still in the development stage. The pressure-molding process is in a very elementary stage of development and does not offer the same possibilities as the casting.

It is clear that there is much to be learned concerning fundamental aspects of solid-fuel[5] rocketry. The work reported here represents only the beginnings of extensive research programs that are planned under the auspices of the Army and Navy.

[2]The solvent is removed by drying at elevated temperatures in forced-air-drying houses.

[3]In operating up to 6 presses at the Eaton Canyon facility in a four-year period, 3 shifts a day, in which nearly 8,000,000 pounds of ballistite were extruded, the Section L–Cal. Tech. contract group had only 2 press blowups. One was reported at the Naval Powder Factory, Indian Head, where 5 presses were operated. In none of these was there any injury to personnel, thanks to proper construction and safety precautions.

[4]See *Reader's Digest,* April 1945, "Hell's a-Poppin in Kansas" by Paul W. Kearney, condensed from *Chemistry* (Science Service, Inc., 1719 N St. N.W. Washington, D.C.).

[5]The Division 3 programs were concerned primarily with rockets making use of the available ballistite propellant. Work in new propellants was carried on under the Explosives Division (8) of NDRC with which Division 3 co-operated. Division 8 had an experimental group located at the ABL plant.

# APPENDIX 4

# *PROPELLANT PANEL*

In 1943, June 30, a Propellant Panel was established by the Joint Army-Navy Committee on New Weapons and Equipment to discuss powder research and development and determine the directions in which progress needed to be made.

The membership of the Panel was:

R. E. Gibson, Chairman, Director of Research, Contract OEMsr-273, Section H — Division 3;

B. H. Sage, Supervisor Propellant Section, Contract OEMsr-418, Section L — Division 3;

L. P. Hammet, Member, Division 3 and 8, NDRC;

and the following Service Representatives:

Colonel Luke, Colonel Osborne, and Commander Sides.

The three civilian members and Colonel Luke remained on the Panel for the duration of the war. However, changes were made frequently in the other Service representatives and the number of officers assigned to the Panel was increased by two.

Division 3, through its contracts, supported investigations[1] for the panel and provided staff assistance for the Chairman; J. W. Burns of the GWU contract group was Assistant to the Chairman. Consultants to the panel were Dr. Marcus Hobbs of Duke University, B. L. Crawford of Minnesota, and Dr. B. B. Owen of Yale University.

An examination of its reports shows that among the problems considered were:

1. Stability, surveillance, and storage of powder.
2. Burning properties of propellants.
3. Composite propellants.
4. Internal ballistics.
5. After-burning of rockets and effect on fuze functioning.
6. Ballistic performance of various webbed grains.
7. Stresses in Service uses.
8. Measurement of physical properties.
9. Calorific data and heats of explosion.
10. Methods of analysis — chemical, chromatographic, and spectrophotometric.
11. Fabrication of large grains (a) by pressure molding of ball powder and (b) casting double-base propellants (still at experimental level at end of war).

[1]Investigations for the panel were also carried on under the auspices of the Army and Navy.

12. Fabrication of large grains by consolidation of fine-cut, smokeless powder using explosive plasticizer (in pilot scale at end of war).
13. Use of slow-burning, dry-extruded, double-base powder. (Demonstration of satisfactory performance by end of war.)
14. Studies both short-range, empirical, and long-range on kinetics of burning of double-base powders.

# APPENDIX 5

## *SUBCONTRACTORS UNDER OEMsr-418*

Altogether, the California Institute group, during the course of its work under OSRD Contract OEMsr-418, had some 400 subcontractors. Unfortunately, limitations of space prevent listing them all, with an acknowledgment of their individual contributions. As a compromise, and with apologies to those who are omitted, a small number are listed below whose achievement was outstanding, either in volume of production or in assistance in problems of design, tooling, and the like.

| | |
|---|---|
| Audio Products Company<br>Burbank, California | Launcher firing switch stopper relays |
| Axelson Manufacturing Company<br>Vernon, California | Outstanding work on 5-inch HVAR program |
| Bakewell Aircraft Products Co.<br>Los Angeles, California | Excellent high-quality work on parts of many programs |
| Bermite Powder Company<br>Saugus, California | Fuze loading |
| Bewley Allen<br>Alhambra, California | Best source for fuze manufacturing in the area |
| Bohman & Schwartz Coach Builders<br>Pasadena, California | Continuous assistance on many experimental machining programs |
| Bunch & Bunch<br>Los Angeles, California | Valued assistance in sandblasting and heat treating |
| Carbonic Specialties Company<br>South Pasadena, California | Fine experimental machine shop and good performance on first aircraft-rocket program |
| Chicago Engineering Company<br>Pasadena, California | Best source of hot-spun single-piece nozzles |
| Coast Centerless Grinding Company<br>Los Angeles, California | Grinding work of all types |
| Consolidated Steel Corporation<br>Los Angeles, California | Steel fabricating on launcher programs |
| Cook Heat-Treating Corporation<br>Los Angeles, California | Best heat-treating source |

| | |
|---|---|
| Cressey Machine Works<br>San Pedro, California | Good general machine shop |
| Crown City Lumber & Mill Company<br>Pasadena, California | Great help on crates and boxes |
| Crown City Plating Company<br>Pasadena, California | Continuous assistance on all plating problems |
| Davidson Manufacturing Company<br>Los Angeles, California | Fuze-manufacturing source |
| General Machine Works<br>Los Angeles, California | Work on 4.5-inch BR and SCAR nozzles |
| Hanes Paint & Body Shop<br>Pasadena, California | Best painting source |
| Harlow Aircraft Company<br>Alhambra, California | Launcher engineering and fabrication |
| Joshua Hendy Iron Works<br>Sunnyvale, California | Excellent work on 5-inch spinner and *Camel* programs |
| Fred C. Henson Company<br>Pasadena, California | Fine instrument work |
| Kenneth C. Holloway<br>Pasadena, California | Continuous assistance in difficult sheet-metal forming |
| Hydril Corporation<br>Los Angeles, California | Development of SCAR and aircraft-rocket nozzle-making machinery |
| Kelman Electric & Mfg. Co.<br>Los Angeles, California | Electric circuit work on many programs |
| Kittle Manufacturing Company<br>Los Angeles, California | Assembly of retro-fired antisubmarine bomb |
| Lane-Wells Company<br>Los Angeles, California | Mousetrap and fuze work |
| Metalite Manufacturing Company<br>Los Angeles, California | Excellent source of spinnings and stampings |
| Midwest Piping & Supply Co., Inc.<br>Los Angeles, California | Source of high-quality forgings |
| Mills Iron Works<br>Los Angeles, California | Source of forgings |
| Mount Wilson Observatory<br>Pasadena, California | Experimental machine shop |

| | |
|---|---|
| Oedekerk & Ludwig<br>Pasadena, California | Manufacturing of SCAR and 5-inch spinner rockets |
| Pacific Screw Products, Inc.<br>Southgate, California | Source of high-quality screw machine parts |
| Tirrill and Tirrill<br>Pasadena, California | Construction assistance and boxes |

PART TWO

# Terminal Ballistics

*The History of Division 2, NDRC*

# *ACKNOWLEDGMENT TO PART TWO*

This history of Division 2 is principally the work of John E. Burchard, the first Chief of the Division. In this task he was assisted by Burnham Kelly.

Chapter XXX was based upon the admirable longer history of the trainer bullet project provided by P. M. Gross. Chapter XXVIII was prepared from contributions by a number of the men closest to the individual projects. R. A. Beth provided the material on terminal ballistics of concrete; A. H. Taub and myself separate portions of the shock wave section; David Mayer wrote about earth shock and M. P. White about rapid strain; L. G. Smith furnished the material on plastic protection, E. M. Pugh that on defense against shaped charges, and C. W. Curtis that on hypervelocity. The muzzle-blast contribution was from J. J. Slade, the Data Sheet section from R. J. Slutz, and the discussion of the operations analysts training program from J. G. Stipe, Jr. V. Rojansky described the AN-23 project.

To get the facts straight, give credit where credit is due, criticize where criticism now may prevent similar errors in the future, and, finally but not least, make the whole interesting and readable — these are not easy tasks. In my opinion Professor Burchard has done all these things with great success. For being willing to undertake the job and for succeeding at it, I should like to express to him my very great appreciation. I should also like to thank Professor Kelly for his important share in this project, and all the contributors listed above for their invaluable help.

As Division Chief at the time of writing of this history, it was my duty and privilege to read, criticize, and approve it.

E. BRIGHT WILSON, JR.

Cambridge, Mass.
June, 1946

# CHAPTER XXVI

## *KNOWLEDGE IS A WEAPON TOO!*

IN 1940 war began for the National Defense Research Committee. The shadow of the Luftwaffe fell long across the world and not alone on Britain. No one knew what bombs would do but everyone expected the worst. People wrote pseudo-scientific books about Structural Air Raid Precautions suggesting shelters based on the death-traps the British had then thrown up for lack of better information. This better information British scientists and engineers were feverishly seeking, but the public and the writers did not know that.

Americans who worried about these matters focussed their attention upon the Panama Canal. Here, they would say, was the critical chink in our armor; here was where the Eastern or the Western foe would strike. The command of the Panama Canal Zone was testing a structure made up of a grillage of steel beams and armor plate, dropping hundreds of bombs to effect a single test hit; meanwhile, the Air Force held to the view that our planes equipped with the Norden sight could drop bomb after bomb with monotonous regularity into a proverbial pickle-barrel; and thought, too, that any bomb weighing 100 pounds and carrying 50 pounds of amatol would strike a telling blow.

When people worried at all about measuring what a bomb might do, they bored holes of different sizes into a plywood board, stretched paper of more-or-less uniform properties across the holes and evaluated the intensity of the blast from the diameter of the smallest hole over which the paper was broken; or if they were of the Navy, they held up a cylinder with a piston in it attached to a piece of string; the blast would drive the piston in and the ensuing suction would draw it back. The length of the loose string would give some measure. Effects of blast against humans were found by having a group of volunteers step up, bit by bit, to the closest point at which they could stand the muzzle blast from giant naval rifles.

Persons interested in the curiosities of science would have known of Charles E. Munroe, American chemist working in 1888 at the Newport Naval Torpedo Station, who had written then about the shaped charge and reported that a hollowed piece of explosive could sometimes accomplish more than a solid one; if one put a leaf between the charge and a piece of steel, one could imprint the leaf shape in the hard alloy. This was interesting but would not suggest that the Munroe effect would give both

sides a lethal antitank weapon. These same curiosity seekers might have found, in an old Ordnance journal, papers by Gerlich insisting that, by tapering the inside bore of a gun towards the muzzle, much higher than normal velocities would be produced and that these hypervelocities might be useful; or have read of early U.S. Army Ordnance rejection tests; before the end, guns based on this principle were wreaking havoc for Rommel in Tunisia, and after one was captured American work, which had already been started, was enormously accelerated.

Regarding these extra-high velocities, one could without difficulty have assembled experts to affirm either that they were useless since no projectile could stand up under them, or that they were perfection itself, since even a piece of butter could be driven through armor plate if fired at a high enough velocity.

If one had been concerned as to how much concrete it would take to stop a bomb or a projectile, he might have looked long in the literature to find incomplete speculations by the Italian di Giorgi, the German Vieser, the Czech Skramtajew and the older and greater Euler. None of these would have helped him much, and he would have drawn more, though still scant, comfort from the only available scientific tests, conducted by the French Metz Committee of 1835 and analyzed subsequently by Poncelet. But neither the projectiles, the guns, nor the concrete of 1835 could be looked upon as reasonable prototypes of the corresponding things in 1940.

No one would have thought that a sort of macadam, a combination of stones and asphalt, would, when wrapped around turrets and bridges, become standard protection for merchant vessels against strafing machine-gun fire with many a seaman's life saved thereby; much less would it have been believed that conventional methods of comparing armor would have proved utterly unreliable for evaluating this plastic material.

Least of all might it have been suspected that the day would come when the split-second timing of air warfare would make the gunner useless who had learned how to shoot by firing at towed targets and have made it imperative to create a training device whereby a gunner in a bomber in flight could fire a real bullet from a real gun at a real fighter plane which was attacking the bomber, score, register and count his hits — and all without harm to fighter plane or fighter pilot.

In the transition from this early situation to the knowledge of 1945, Division 2, NDRC, was to play a leading though almost never a solo part. All of its work depended in large measure upon the closest of co-operation with other Divisions of NDRC, with the Army, the Navy, the Air Force and with our brilliant scientific allies, the British. This history is written from the point of view of Division 2; but it will be false if it gives the impression that the job which was done was other than a team job.

Though it did not procure weapons of search or of demolition, the work did not lack its spectacular quality. Moreover, it usually did affect the

operational use of the major weapons of the war, which despite the brilliance of rocketry remained the machine gun, the cannon, and the bomb. When the division closed its books, the situation had changed from the calm days of 1940. It was possible to predict with adequate military accuracy what a projectile of any mass, velocity, shape and angle of impact might be expected to do to a piece of concrete of any assumed thickness, physical properties and reinforcing; it was possible to select, from experience and from the knowledge of what bombs would do, the probability of hitting near a target and, from the characteristics of the target, what sort of bomb load should be carried; for the paper and tube gauges there had been substituted sensitive electronic devices to measure the peak pressure as a function of milliseconds, and all this gear was mounted in full-size truck laboratories which could move to any proving ground in the country; with this equipment ways of increasing the effectiveness of bombs manyfold had been shown; special defenses for our tanks against shaped charges were in process when the total weight of our attacking forces reduced the importance of the problem; extensive development had taken place in the knowledge of hypervelocity and the problem of the shattering projectile which so often accompanied this greater speed; a technique for comparing various plastic armors had been developed which was sensitive and reliable; and the magic bullet, with which to train the gunners of B-17's, B-24's and B-29's, had been proved and was being fired on our training grounds to the extent of millions of rounds a month.

From the laboratories which were doing these things, a steady stream of young men, recruited and trained for the purposes, had entered the services of the Air Force as civilian operations analysts with the Bomber Commands in every theater, in India, in Burma, in China, in New Guinea, in the Aleutians, in the United Kingdom, in Africa, in Italy, in France, in Germany, everywhere where a bomber force of any size was based, and were applying daily an engineering approach to the selection of the weapon or combination of weapons which, in view of the job to be done, would wreak most havoc on the enemy.

It is the pursuit and application of new knowledge to military operations which is the history of Division 2 and not the development of any special weapon. The contribution of the division cannot be measured on a single field of battle, nor can it ever be claimed to have turned the tide of any affair. Its story then cannot appropriately be told in headlines; for work of this sort it is more significant for history to know the methods by which the work was conducted, the basic difficulties which stood in the way of still more solid accomplishment, and to understand what was finally achieved and what it signified. This is the sober tale these pages will attempt to unfold. To open the tale requires a somewhat pedestrian account of administrative organization which the next chapter will attempt to dispose of as briefly and as painlessly as possible.

# CHAPTER XXVII

## THE TEAM

IN THE ORIGINAL organization of the National Defense Research Committee, there were four divisions. One of these, Division A, Armor and Ordnance, was under the direction of Richard Chace Tolman.[1] The division had a number of sections, two of which are pertinent to this history as they were ultimately merged into Division 2 in the reorganization of NDRC, which took place December 9, 1942.[2]

The earlier of these Sections, Section S, was created in the summer of 1940 under the Chairmanship of Henry DeWolf Smyth.[3] Concerned at the start, as were all with access to the facts, with defense against the formidable striking power of the Axis, Smyth tackled problems of *Terminal Ballistics,* which means in lay language, what happens when a projectile or bomb strikes its target. His primary interest was armor and, since this had long been the concern of the Ordnance divisions of both Army and Navy, Section S was designed at the outset to fill gaps and to pioneer in new techniques, rather than to have a full and independent armor program of its own. Careful studies of the effects of impact and of high pressure were undertaken at the suggestion of the special Army-Navy-OSRD *Ad Hoc* Committee on Armor Plate. When scientists began to crowd the Service ranges, Smyth responded to the suggestion of the Naval Research Laboratory and started construction of a civilian range at Princeton. This range, built behind the Palmer Stadium and frequently blazing away to disconcert the football squad, ultimately grew to the length of 300 feet, accommodating targets weighing up to 20,000 pounds and guns up to 37-mm. caliber, with projectiles fired at velocities up to 5,000 feet per second.

The other section, Section B, was created September 10, 1940, with the title *Structural Defense,* under the Chairmanship of John Ely Burchard.[4] At that time, Burchard was already engaged in directing the study of both

[1]Dean of the Graduate School, California Institute of Technology, and Vice-Chairman of the NDRC.

[2]The reasons for this reorganization will be found in the volume *Organizing Scientific Research for War.*

[3]Chairman of Physics at Princeton University and later of atomic bomb fame.

[4]Architectural engineer and Director of the A. F. Bemis Foundation at the Massachusetts Institute of Technology.

Army fortifications and civilian air-raid shelters from the vantage point of the new scientific techniques,[5] and the role of the section at the start was primarily that of liaison with this closely related work. It was considered that the studies might expand beyond the purely defensive interests of the Army and that additional facilities and personnel might then be required from the NDRC. This soon proved to be the case.

As time went on it became apparent that the problems of Sections S and B touched at many points, that the facilities they would require were often common facilities, that the personnel engaged to work for the two sections could profitably be interchanged from time to time and, hence, that a single direction would be more efficient. Smyth was already beginning to become concerned at Princeton with work which would bear on the ultimate production of the atomic bomb and, accordingly, on December 15, 1941 the two sections were merged into Section B, with the title "Structural Defense and Offense" and under the chairmanship of Burchard.

On December 9, 1942, NDRC reorganized, and Section B of Division A became Division 2 of NDRC with Burchard as Chief. The new organization remained essentially intact until June 1944, when Burchard resigned to become Assistant Chief of the Office of Field Service. His successor was E. Bright Wilson, Jr.,[6] a natural choice because, in his work for Division 8, NDRC, he had been concerned with measuring the effects of explosions principally in water but also in air, and the latter had always been a primary concern of Division 2 as well. At the time of this change in top divisional administration, Wilson's work for Division 8 was largely transferred to Division 2.[7] Wilson remained Chief of the Division until March 1946 when he was succeeded by Dr. Eugene W. Scott who dealt with the remaining problems of liquidation.

Since there were no radical changes in policy, interest, or activity in the various phases of administration, this history makes no special effort to discriminate between what took place under the different administrators, although the dates provided will afford a rough guide to this discrimination for those who are interested.

Section B of Division A[8] was really managed by a triumvirate consisting of the Chairman and Professors H. P. Robertson and Walker Bleakney of Princeton University. Robertson[9] was primarily concerned with the theoreti-

[5] As Executive Officer of the Committee on Passive Protection Against Bombing, created by the National Academy of Sciences earlier in 1940 at the request of the Chief of Engineers, U.S. Army.

[6] Associate Professor of Chemistry at Harvard University and at the time of this final reorganization Research Director of the Underwater Explosives Research Laboratory of Division 8 at Woods Hole, and a Member of both Division 2 and Division 8.

[7] Namely that at Woods Hole and that of the closely related contracts at Cornell University and at Stanolind Oil & Gas Company.

[8] And for some time Division 2 of NDRC as well.

[9] Professor of Mathematical Physics (Relativity, Mechanics).

cal aspects of the work and with administration at the policy level; Bleakney,[10] with experimental aspects and with direct charge of operations of the Princeton University Station.[11]

Bleakney remained a principal factor in the active work and was Deputy Chief of Division 2 to the end of the war. After Robertson's tour of duty with the 8th Bomber Command, on the other hand, he became increasingly interested in foreign service and by the summer of 1943 had effectively taken up a residence in the European theater which extended until December 1945. During the early part of this residence he was attached to the Liaison Office in London as a special representative of Division 2[12] and he went on to distinguished service with the War Department, first as Scientific Adviser to USTAAF [13] and ultimately as Chief of the Scientific Branch, FIAT,[14] and as Scientific Adviser to the Commanding General U.S. Group Control Council.[15] These major activities of Robertson are not a proper part of this history. Until the end of his work he continued to maintain an interest in the work of Division 2 and to influence its thinking. Indeed, Wilson made very few changes in administrative personnel. The Special Assistant to the Chief, Burnham Kelly,[16] and the two Technical Aides, Ralph J. Slutz[17] and Merit P. White,[18] were all retained in their posts. The only important reorganization, caused principally by administrative regulations from OSRD headquarters and the addition of the Woods Hole work, was effected in the lists of Members and Consultants.[19]

The division's first title, "Structural Defense and Offense," was *faute de*

[10]Associate Professor of Physics (Ionization in gases, isotopes, nuclear physics).

[11](The principal contractor for Division 2); he was also the representative of the group in its liaison with other Divisions such as Division 1 and Division 8.

[12]Robertson's activities at the Liaison Office in London are treated in the volume *Organizing Scientific Research for War.*

[13]United States Strategic Air Forces under the command of Lieutenant General Spaatz.

[14]Field Information Agency Technical USFET (United States Forces European Theater).

[15]Lieutenant General Lucius Clay.

[16]City planner and lawyer, who had been in Washington since 1941 as Assistant Executive Officer of the Committee on Passive Protection Against Bombing and later Technical Aide of Section B. He served as Washington representative until November 15, 1944, when he resigned to join the ALSOS Mission in Europe.

[17]Candidate for Ph.D. at Princeton University, early assistant to Robertson in his literature survey and in charge of Divisional Intelligence.

[18]Assistant Professor of Mechanics at Illinois Institute of Technology at the beginning of the war and finally in charge of work on rapid rates of strain and editor of Divisional scientific reports.

[19]Members of Division 2:

Under Burchard:

| | | |
|---|---|---|
| R. A. Beth | C. W. Lampson | F. Seitz, Jr. |
| H. L. Bowman | W. E. Lawson | A. H. Taub |
| C. W. Curtis | H. P. Robertson | E. B. Wilson, Jr. |

Under Wilson:

| | | |
|---|---|---|
| H. L. Bowman | D. P. MacDougall | J. von Neumann |
| W. E. Lawson | S. A. Vincent | |

*mieux* a misnomer. The primary interest lay in the efforts of bombs on targets of all sorts and on certain limited and basic effects of projectiles on all materials, especially concrete and armor.[20] The work on armor led to more important studies of the effects of a new weapon, the hypervelocity gun,[21] and to a new treatment of the ballistic performance of an important expedient for the protection of merchant vessels, plastic armor.[22] This naturally led as well to an effort to improve existing forms of plastic armor with interesting results.[23] The work on defense of structures against bombing naturally led to an evaluation of the bomb as an offensive weapon and then, since the division had developed the largest pool of information on this subject in the country, to an extensive program of data sheets for the use of bomber groups of the Air Forces.[24] More profound studies of the performance of targets against impact, both as regards perforation and penetration and as regards the vibration effects in the mass structure created by the impact, led to fundamental studies in high pressure,[25] rapid rates of strain in wires and plates,[26] theory of dynamic deformation,[27] effect of im-

Consultants to Division 2:

Under Burchard:

| | | |
|---|---|---|
| C. W. Barber | E. N. Gelotte | V. Rojansky |
| H. F. Bohnenblust | J. W. Greig | A. C. Ruge |
| P. W. Bridgman | P. M. Gross | W. M. Rust, Jr. |
| W. T. Brightman, Jr. | R. J. Hansen | H. D. Smyth |
| L. A. Brothers | J. G. Kirkwood | O. J. Stephens |
| J. S. Burlew | E. H. Land | R. Stephens, Jr. |
| R. W. Carlson | R. R. Martel | N. J. Thompson |
| D. S. Clark | A. Nadai | T. von Karman |
| L. A. Delsasso | N. M. Newmark | B. B. Weatherby |
| J. W. Dunham | C. L. Post | J. B. Wilbur |
| W. M. Fife | F. E. Richart | |

Under Wilson:

| | | |
|---|---|---|
| R. A. Beth | W. D. Kennedy | W. M. Rust, Jr. |
| P. W. Bridgman | J. G. Kirkwood | F. Seitz, Jr. |
| R. H. Cole | C. W. Lampson | L. G. Smith |
| P. C. Cross | R. R. Martel | A. H. Taub |
| C. W. Curtis | Stanford Neal | T. von Karman |
| P. M. Fye | N. M. Newmark | B. B. Weatherby |
| P. M. Gross | E. M. Pugh | J. B. Wilbur |
| M. E. Hobbs | V. Rojansky | |

[20]Terminal Ballistics of Concrete and Steel and certain studies of performance of models at Princeton University under Beth, Curtis, and Lampson; effect of coned charges on various materials at Carnegie Institute of Technology under Pugh.

[21]Under Curtis at Princeton University; the gun itself was developed by Division 1 (see pp. 343–423).

[22]Under Smith at Princeton University.

[23]At Polaroid Corporation under Land.

[24]Under Slutz, then Freeman at Princeton University.

[25]At Harvard University under Bridgman.

[26]At Carnegie Institute of Technology under Seitz; at California Institute of Technology under von Karman and Clark; at Massachusetts Institute of Technology under A. V. de Forest; at Westinghouse Electric & Manufacturing Company under Nadai.

[27]At Massachusetts Institute of Technology under Wilbur; at Cornell University under Kirkwood.

pact on individual structural members.[28] The same considerations, plus the need of knowing more about the assessment of a new bomb or explosive as an attacking weapon, led to fundamental studies of shock waves in air,[29] in earth, and in water.[30] From the experience in the study of shock waves, it was natural in turn to be called upon to study the suppression of blast from the muzzles of high-power guns,[31] a very important operational problem. Incidental to the attempt to estimate these forces, substantial studies of measuring devices were carried on, including crusher gauges for measuring the pressure in gun barrels,[32] and various electronic and other apparatus for the measurement of blast.[33] The latter resulted in the development of a large mobile laboratory so that measurements could be taken anywhere in the field and the subsequent building of a second such apparatus for the Aberdeen Proving Ground.[34] A unique problem, placed in the Division because of its familiarity with the ballistics involved, asked for the development of a bullet which could be fired realistically from flexible guns in planes in flight at actual fighter planes also in flight, thus closely simulating combat.[35] This last represented the only contribution of the Division to the design of weapons as a full-fledged divisional project, although the information developed by the division, of course, conditioned many other designs. The only other contribution of a materiel nature was the mobile laboratory and the associated measuring devices.

Thus, in contrast to a number of divisions of NDRC, Division 2 was primarily an information division and one of its principal problems was to get this information fully and rapidly into the hands of the users in the field. In addition to the data sheet department,[36] this led to the development of a training department for the production of operations analysts[37] to serve all the bomber commands of the Air Force, and the establishment of a special project, jointly with Division 11 and the Applied Mathematics Panel, to study the appropriate allocation in a bomb load to high explosive and to incendiary bombs.[38]

[28] At University of Illinois under Richart and Newmark.

[29] At Princeton University under Lampson.

[30] At Woods Hole under Cross; at Cornell University under Kirkwood.

[31] At Princeton University under Taub and J. J. Slade; at General Electric Company under E. L. Robinson; at California Institute of Technology under Clark D. Millikan; at Franklin Institute under H. B. Allen.

[32] At Carnegie Institute of Technology under Seitz.

[33] At Princeton under Lampson; at Stanolind Oil & Gas Company under Daniel Silverman; at Humble Oil & Refining Company, under Rust, and Geophysical Research Company, under Weatherby, both without NDRC contract; at Woods Hole Oceanographic Institution under Cross.

[34] Built by Herbach and Rademan, Philadelphia, under the general direction of Lampson at Princeton.

[35] Principally at Duke University under Gross.

[36] At Princeton University.

[37] At Princeton University.

[38] Project A–N 23 under the general direction of V. Rojansky, under contracts with Princeton University, Arthur D. Little, Inc., University of California, and with the assistance of the Office of Field Service.

Because Division 2 was an information division, it was important that it receive all pertinent information, as well as stand prepared to give it. Early contact was established with the British. The first mission on behalf of Division 2 was undertaken by Burchard and Robertson in the early fall of 1941, and they returned to the United States just before Pearl Harbor after two months in the United Kingdom. They effected direct liaison with Sir Reginald Stradling[39] and other key persons[40] in England, which remained effective to the end. They also brought back the first substantial quantity of British reports and insured the continued flow of such reports. They brought back specimens of British gauges for the measurement of blast and instituted the beginning of this work in the United States, so far as Division 2 was concerned. Burchard, in particular, interested himself as well in the effects of German bombing on individual building types and, with the aid of the splendid reports by the Regional Technical Intelligence Officers of the Ministry of Home Security directed by Stradling, established the data sheet program. Robertson became deeply interested in operational research as being developed by the British under Blackett and others, and this early interest, maintained during the months which followed, made him a prime force in the development of operational analysis in this country.

The second expedition was undertaken by Bleakney and Professor H. L. Bowman in August of 1942. Bowman extended the survey Burchard had made on building types; Bleakney continued the survey work necessary for general Divisional planning and concentrated particularly on details of laboratory technique. He brought back with him extremely valuable information concerning apparatus for the study of impacts and explosions; these details played an important part in facilitating the rapid development of the experimental programs of Division 2.

In the late fall of 1942 Robertson returned to the United Kingdom on loan to the Operational Research Group of the 8th Air Force. While engaged in this activity he maintained an active interest in the work of the Division and a steady and informative correspondence. He returned to the United States early in 1943.

By this time it was evident that the Division needed full time representation in London. Consequently, in the summer of 1943, Robertson made his third trip to England, where he assumed the post of Divisional representative in the London Mission, together with other duties of a similar nature for other divisions. At about the same time, Norman C. Dahl, an employee of Princeton University, spent six months working at the Princes Risborough station of the Ministry of Home Security on problems of concern to the data sheet program. Slutz, Technical Aide, was despatched in the

[39] Chief Scientific Adviser, Ministry of Home Security.

[40] The debt to our British colleagues can scarcely be overestimated. High on any honor roll of Division 2 would stand the names of Stradling, Professor J. D. Bernal, Professor Solly Zuckerman, Professor W. N. Thomas, Professor J. W. Baker.

fall of 1943, partly to replace Dahl, since it was believed that more was gained by a rotation of personnel in England than by a continued stay. On Robertson's temporary return to the United States in December 1943, arrangements were made whereby he would go on to the staff of General Spaatz. Slutz, therefore, took over Robertson's Divisional obligations in England and remained in that capacity until May 1944, when he was replaced by White, Technical Aide, who remained in Europe until the end of 1944.[41]

The considerable flow of information from Great Britain needed to be matched by a similar flow of information from the Service and OSRD laboratories in the United States. Experience soon showed that this could not be left to chance. Slutz was therefore charged with this responsibility. The information received was so comprehensive that in turn its circulation and classification had to be organized, so Slutz undertook the establishment of a full-fledged library located in Fine Hall at Princeton University, a system of classification of the contents by subject and author, a system of circulating of the pertinent material to appropriate recipients.[42] This library became one of the three distinguished document rooms of scientific information established by the divisions of NDRC; it was frequently visited by other OSRD personnel and by students from the Services; at the close of business it was considered valuable enough to be transferred intact to the Liaison Office as one of the important components to be delivered to the agency successor to OSRD.

The contract business of Division 2 followed the usual pattern of NDRC and needs no particular discussion here.[43] The majority of the contractors had single projects but, like some other Divisions, Division 2 had until June 1944 one contractor whose efforts were major in many fields and who on the average received well over half of all the appropriations for the Division.[44] This was the Princeton University Station, under the direction of Bleakney. As the business of the Station developed, it became evident that full-time competent business management was required to permit Bleakney to devote his attention to the scientific matters from which his mind could ill be spared. Accordingly in the spring of 1943, H. L. Beckwith[45] was

[41]Many others went to Europe, particularly England, but also the continent after D-Day in whole or in part on behalf of the Division. E. B. Wilson, Jr., was in England at the time of Robertson and Burchard's first visit but primarily on affairs of Division 8 with which he was then principally associated. Others who visited Europe at one time or another were Seitz, Lawson, R. H. Dietz, Taub, Beth, and Bleakney, who made a second trip in the summer of 1945.

[42]Under Betty Meeker and Marjorie Burchard.

[43]General contract procedure is discussed in the volume *Organizing Scientific Research for War.*

[44]In July 1944 the Underwater Explosives Research Laboratory at Woods Hole was transferred from Division 8 to Division 2.

[45]Associate Professor of Architecture at M.I.T.

appointed executive officer of the Princeton Station and took up full-time residence at Princeton. The general course of the work at the Princeton University Station was administered by an informal steering committee consisting of Bleakney as Chairman, Beckwith, Beth, Curtis, Lampson, Smith, and Taub. For this station it is no exaggeration to say that without it Division 2 would not have been a Division. The history which follows will not, however, be a history of the Princeton University Station, but rather a history of project development, which we may now explore.

# CHAPTER XXVIII

## *FROM DEFENSE TO ATTACK*

THE WORK of Division 2 was, in the last years of the war, directed primarily at offense. None the less it began with purely defensive studies, and the growth from this start is the history of the division.

In the first instance, those who were worrying about bombing were legion and their ideas numerous and often fantastic. There was, for example, the manufacturer who conceived the idea that an enormous net of steel cables stretched over the Panama Canal would stop and toss off bombs in exactly the same way that a circus net arrests the fall of the trapeze performer. He had backing from some Congressmen, all sincere but none of them very scientific. He had supported his contentions with the detailed calculations of a university professor who had made large and erroneous assumptions. Division 2 scientists attempted to explain, with calculations of their own, that the impact velocity of the bomb was so much greater than that of the circus performer that conditions would be entirely different, that the bomb would have passed through the net before the more remote members of the structural system even knew the bomb was there, so to speak. Under these circumstances, very little energy could be absorbed by the system, and the remaining energy would be far in excess of that needed to defeat the net. The sponsor's backing was sufficient, however, to cause an actual trial to be made at the Aberdeen Proving Ground. It was out of the question to build a full-size net and to hit it with a full-size bomb. In fact, earlier experience had shown that bombing was a wasteful way to experiment, and that more accurate results could be obtained much more rapidly with the better aim possible when the target was tilted up vertically and the bomb converted to a projectile of similar mass and shape fired with comparable velocity from a gun. It took some time to persuade the sponsors that a model of the net at ⅓ scale using an 8″ projectile to simulate the 2000-pound bomb would really be informative. Finally the experiment was set up. The net was suspended over smooth-running pulleys. If then the end cables of the target received any stress at all as a result of the impact, the net would move. An impressive group of observers were assembled. With some difficulty the projectile was dribbled out of the gun at the lowest possible velocity of 800 ft. per second. This was lower than bomb impact velocities and more favorable to the net. Nevertheless, the first impression the observers had after firing was that trees were crashing to the ground a mile or so down

the range. There was a neat hole in the net, and the cables had not moved any measurable amount, however small. This settled the net.

A Philadelphia helicopter expert proposed the still more elaborate device of an enormous mast with cables 1000 feet long rotated in the horizontal plane at such velocity as to cut a falling bomb in two and throw it to one side. He demonstrated this by dropping paper clips in an electric fan in the Division Chief's office. This proposal was sidetracked by calculations alone which satisfied the inventor.

As time went on, the number of proposals of this sort dwindled and the Division could concentrate all its interest on more serious matters.

## Block-Busters Versus Concrete

The general problem of defense could be divided into two parts depending upon the quality of the target to be defended, one relating to the usually weak civilian structure, the other to the usually strong military structure. The latter might be a great shelter or fortification. Although the War Department had conducted early experiments on grillages of steel beams and armor plate, it was obvious by 1940 that steel would go to other purposes than fortifications and that concrete would be the material of principal interest. It was accepted as a hypothesis that any bomb which actually penetrated into a fortification and went off would do heavy damage. The bomb must therefore be kept out of the fortification, and the problem was to find out the amount of concrete required to do this. This led to an extensive study involving projectile shapes and weights, nose effects, impact velocity and angle of impact for the projectile, deformability of the projectile, properties of the concrete in the target, thickness of the concrete, and nature of the reinforcing used. Altogether this became one of the principal programs of the Division and was carried on from beginning to end under the leadership of Dr. R. A. Beth.[1]

Work on the terminal ballistics of concrete was carried on in close association with the Committee on Passive Protection Against Bombing (later the Committee on Fortification Design) of the National Academy of Sciences and the Corps of Engineers, U.S. Army.

Much to everyone's surprise, it turned out that very little fundamental knowledge was available on projectile and bomb penetration into concrete, and, indeed, that the work on general theories of penetration (studied and admirably summarized for the Committee on Passive Protection Against Bombing by Robertson) had achieved only moderate success even in accounting for the experimental observations on steels and armor. The latter had, at least, been subjected to more or less continuous study through the years. As for concrete, most interested parties tacitly assumed that someone else

[1]Supported at various times by J. G. Stipe, Jr., J. T. Pittenger, G. T. Reynolds and E. J. Schaefer.

must know about its terminal ballistic behavior. Actually this was not true. Almost no experimental data were available. The principal previous efforts at systematic experimental study of materials other than steel dated back to the work of the Metz Committee (G. Piobert, A. Morin and Is. Didion) of 1835, in which balls were shot into various media, ranging from damp clay through various woods to limestone, and to the studies of Nobile di Giorgi, about 1910, based on mediocre data in which striking velocities and other important parameters had to be estimated. However, di Giorgi's work was important in that it represented the first breakaway from theoretical to empirical formulas for penetration.

A serious study of concrete penetration evidently had to be undertaken. From small beginnings in late 1940 it grew to a program of considerable size with many ramifications. Almost from the beginning, two decisions were made which guided a great deal of the work and which in the end contributed much to its value. (a) Work at model scales with small calibers was begun almost immediately at Princeton, and (b) it was decided to study the effects at all velocities which could be obtained with the available guns[2] rather than just in the narrower range of the striking velocities of bombs.[3] In addition, the aim was to observe quantitatively just what penetrations and other effects were actually achieved, rather than merely to find a safe upper bound beyond which penetrations would not be expected. This lack of restriction to a defensive point of view was, in the end, significant because it later made the results useful for offensive purposes as well.

As in many other war research programs a fruitful co-operation was set up between the military, in this case the Corps of Engineers of the Army, and a group of civilian scientists. The civilians were in a sense "babes in the wood" without any special knowledge of military matters, having been previously interested in things like architecture, cosmogony, and mass spectroscopy. They contributed to the problem a knowledge of Newton's laws and of the calculus, an ability to experiment and to generalize from specific experimental results, and, it must be admitted, unexpected planning, organizational, and even dialectical proficiencies. The Army, besides contributing the problem itself, brought to bear material resources, its years of experience in practical engineering and construction matters, and a willingness to comprehend and go along with projects whose practical implications were not always simple or obvious.

The idea that an expert is a man from out of town, which was held even among some Army officers, was probably helpful to the civilians, particularly at the beginning of the work. It is even probable that some Army Engineers felt that scientists should be able to derive the laws of

[2]Roughly 500 to 3000 feet per second.
[3]I.e. 900 to 1200 feet per second.

penetration from first principles. If so, they were soon disillusioned when the cry went up, especially from the theoretical physicists, for experimental data. But — and this is the point — the Army co-operated wholeheartedly in the various programs that were then set up to get experimental data. In all military research, especially that done under the time urgencies of war, there is risk that too much reliance will be placed upon the "ad hoc" experiment. Such an experiment is one aimed at getting an answer to a specific question quickly without lengthy considerations of its relations to other work or without any added complications to increase these relations. In studying concrete penetration, for example, an "ad hoc" experiment would be one which would determine the effect of a given projectile on a given target over a narrow range of striking velocities. At times such experiments had to be made, as for example when it was necessary to assess the probable protective value of thin concrete slabs against the German 1-kilogram incendiary bomb.

However, in most tests on concrete penetration, an effort was made to use a much wider range of striking velocities and to record full facts concerning both target and projectile characteristics. One of the first fruits of this systematic accumulation of comparable data was an empirical formula for the dependence of penetration on striking velocity, according to which penetrations increase roughly with the 3/2 power of the striking velocity, and which was better than any of the formulas derived from theories of penetration. By June 1941, the intercomparison of such data led to the suspicion, later amply confirmed, that a "scale effect" existed for concrete penetration in the sense that tests with small calibers always gave smaller penetrations, in calibers, than was found with larger projectiles for the same striking velocity and target and under otherwise similar circumstances. This important effect, quite unexpected before 1941, is represented by an empirical factor, proportional to the one-fifth power of the caliber, in all the newer concrete penetration formulas. It is improbable that general results of the kinds mentioned would be obtained by "ad hoc" experimentation, but it is equally obvious from the examples described that experimental work directed at specific questions may be conducted in such a way that the results may have much more general implications as well. As generalizations accumulate, less and less "ad hoc" work is needed, thus saving time and money, because specific answers can be based on the generalizations; "ad hoc" work alone feeds on itself because it makes more "ad hoc" work necessary each time a new question arises.

Early in 1941, the Corps of Engineers had begun aerial bombing tests at Aberdeen on various concrete structures, bombproof shelters and burster slabs. These tests, which also included statically detonated bombs, yielded much miscellaneous information on the behavior of the structures with

respect to blast, earth shock, and direct hit, not to mention the weaknesses against deformation of certain of our own bombs which were uncovered and corrected as a result of these tests.

Very little direct information on penetration and perforation of reinforced concrete walls or roof slabs was obtained because of the difficulty of accurate placing of the striking points with bombs dropped from planes. Even when a hit was obtained, the striking velocity could not be estimated so accurately as was desirable.

Late in June 1941, the Engineers conducted the "AP Bomb Test" at the Aberdeen Proving Ground. Mortar-fired, 1000-pound, 12-inch projectiles were used at a range of 400 feet, so that the impact points could be very accurately predetermined. The principal criticism of the civilian observers was that no provisions had been made for measuring the striking velocities. These were instead to be estimated from the powder load which was adjusted to give a striking velocity of 1000 feet per second. Velocities from 970 to 1040 feet per second were later deduced for some of the shots from high-speed motion pictures taken during the tests. Direct measurements of the striking velocity adds relatively little to the cost of tests of this sort, but a great deal to their value.

Three vertical slabs, 36 inches, 60 inches and 81 inches thick, respectively, were constructed of very good[4] concrete, each slab being 30 feet square with five types of reinforcement — one for each of five shots taken at each target. The 12-inch mortar was brought in on a railway spur track. The slabs were placed so that about 20° obliquity at impact to simulate bomb impact angles would be obtained from the one gun position used. This was done by placing the plane of each vertical slab at 70° to the line of fire as plotted on a map.[5]

All five shots perforated the 36-inch slab, one out of five went through the 60-inch slab while the remaining four produced severe scabbing,[6] and no perforations resulted from the five shots at the 81-inch slab although there was evidence that scabbing was about to begin at this thickness. For the 36-inch slab four residual velocities from 560 to 625 feet per second were obtained by analyzing high-speed movies of the projectile; for the 60-inch slab only two residual velocities were obtained in this way, 55 and 90 feet per second, respectively. There was no clear evidence indicating differences in slab resistance due to the different types of reinforcing. Scratch gauges were attached to the surfaces of the slabs to record maximum and per-

[4]Compressive strength of about 5000 pounds per square inch.

[5]This method seems somewhat simpler than that used in some previous Navy tests at the Dahlgren Proving Ground in which complete bomb shelters were constructed at an angle of 70° to the vertical to simulate with horizontal gunfire the impact angles of bombs. Possibly the Navy had to use the more difficult construction because of the danger from ricochets.

[6]I.e., the spalling off of concrete from the back opposite the point of impact on the front.

manent strains and oscillographic records were made from electric resistance gauges embedded in the slabs to measure internal strains. While the performance and records of the strain gauges gave rise to a number of interesting problems and questions concerning what really did happen, it cannot be said that they shed much light on the behavior of a concrete slab under impact. This illustrates the fact that the art of measurement has not yet caught up with transient phenomena of this type, to be observed under field conditions and in such a complicated mass as a reinforced concrete slab. Nor does theory have very much more to offer when high transient stresses, cracking, and strains beyond the elastic limit are involved.

While much was learned from the AP bomb test, the effects of changes in reinforcing, obliquity, caliber, and striking velocity were still not well enough understood so that the performance of 16-inch projectiles striking the much thicker concrete used in fortifications at higher velocities and at normal incidence could be predicted with confidence. Toward the end of 1941 the construction of four large concrete slabs was begun at Fort Cronkhite near San Francisco for 16-inch projectile tests. The slabs were made 27 feet high and 42 feet wide. Two were made 23 feet thick, one of nominal 3500 pound per square inch concrete, the other of 5000 pound per square inch concrete. The remaining two were made of nominal 3500 pound per square inch concrete and were 16 feet and 13 feet thick, respectively.

During the first few months in 1942, two shots were taken at each slab with a 16-inch, 2100-pound projectile, fired from one of the coast defense guns at the Fort. Normal incidence was used and, to simulate the effect at different ranges, the striking velocities were varied from about 1300 feet per second to 2000 feet per second. Again, no adequate plans had been made in advance to measure striking velocities, but Burchard and Bleakney took out with them a modified Aberdeen-type chronograph from Princeton and the velocities were successfully measured with this equipment by Bleakney.

The first shots were made in January, just one month after Pearl Harbor. They produced two unexpected results which aroused attention and comment. One was that the recoil mechanism of the 16-inch rifle was so damaged by a low-velocity shot that the gun could not be fired at a time when the West Coast was still somewhat exercised over the possibility of a Japanese attack.[7] The other was that a projectile, striking at nearly 2000 feet per second, passed right through 23 feet of concrete, which had been expected to stop it, and emerged with a residual velocity estimated at from 300 to 500 feet per second. This aroused attention because it tended to confirm the existence of a suspected "scale effect" for concrete. Of these two results only the "scale effect" continues to be of significance.

The recoil mechanism was damaged because a smaller than usual powder

[7]Some 700 observers kept the secret!

load was used in a second shot to bring the striking velocity down to 1300 feet per second. The recoil was actually insufficient to bring the protective mechanism, to return the gun slowly to its firing position, into play. The breakdown was kept secret, at least until repairs had been made by flying the necessary new parts out to the Coast. It was possible to adjust the recoil mechanism so that the gun was not damaged again even though striking velocities as low as 1300 feet per second were later used. However, before the mechanism was adjusted for reduced powder loads, certain comments, presumably concerning the relative merits of experiments and National Defense, were offered and satisfactorily answered from Washington.

By the middle of 1941, it had become clear to both the Army and the civilians that it was not always possible to get one's money's worth in practical results of general significance from tests simulating some specific practical situation in every detail. The original bombing trials, the AP bomb test, and even the later 16-inch tests at Fort Cronkhite were all more or less "ad hoc" as planned, and it was not easy to show quantitative connections between the results obtained or to estimate the results to be expected from later tests on the basis of earlier work.

It was rightly deemed to be the role of "science" to establish such relations and to set up general rules concerning penetration and perforation in concrete. Hence, the civilian scientists, at the request of the Corps of Engineers, proceeded to plan the "Penetration and Explosion Tests on Concrete Slabs" which were conducted at the Aberdeen Proving Ground in 1942.

Before discussing this "P&E" test, mention should be made of the small-caliber tests which were started at Princeton late in 1940 and continued until 1944.

The idea behind the small-scale tests was that they would give experimental facts on penetration and perforation of concrete under well controlled laboratory conditions, and that they would serve as model tests for full-scale proving ground work.

The first small-scale concrete targets at Princeton were mixed and poured by hand by Bleakney in September 1940. They consisted of one-foot cubes for penetration tests and of small slabs, one and two inches thick, for perforation experiments. The first testing was done with a standard Army caliber .30 rifle fired by Bleakney in the attic storeroom of Princeton's Palmer Laboratory. Ball ammunition just resulted in smashing the jacketed lead bullet and small penetrations. With caliber .30 A.P. bullets, the jacket was ripped off in the first inch or two of penetration, but often the hard steel core went very deep into concrete after leaving its jacket behind. Striking velocities were adjusted by changing the powder load and were measured from the beginning with an Aberdeen-type chronograph using pairs of metal foil screens which were short-circuited as the bullet passed through. Somewhat later a ballistic pendulum, on which the target block

was placed, was used to measure striking velocities, and an old caliber .45 rifle was secured because the larger caliber was felt to be advantageous.

The behavior of jacketed bullets was unsatisfactory both from the point of view of analyzing the data, and as a model of what might be expected at larger scales. Much work was done in developing satisfactory projectiles that could be made in the laboratory shop in large numbers. After some months a model of the simplest type of monobloc artillery projectile was adopted, made of steel, hardened after machining, and with only one rotating band near the base. A one and one-half caliber radius tangent ogive nose was adopted, and the weight in pounds was made about equal to one-half of the cube of the caliber in inches, thus assuring dynamic similarity at all scales. The attention given to detail in designing these model projectiles turned out to be of great importance when the small-scale penetration and perforation results were compared with large-scale proving ground tests.

When it came to designing model concrete the physicists did not feel competent, and Professor Roy W. Carlson, then of M.I.T., was called upon. Under his direction, and with the co-operation of Professor Tschebotarioff of Princeton, the first so-called "Concrete Properties Survey," to test the effect of concrete properties on penetration resistance, was planned and carried out in 1941, using caliber .45 model bullets and one-foot target cubes. By the middle of the year the accumulated penetration data exceeded the capacity of the then current penetration theories to correlate and account for them. As a result the serious development of empirical penetration formulas for concrete was undertaken, beginning in 1941 with continuing improvement and elaboration until the end of the war. Even now the theory of penetration has not caught up with the empirical formulas based on experimental observations although some progress has been made. Empirical formulas and graphs are now used for predicting penetration and perforation phenomena for concrete, and essentially the same situation exists for steels and armor. It appears that engineers and physicists will need a much deeper understanding of the failure of materials at high stresses and high rates of strain, before the theory of penetration can be dealt with adequately.

The first Concrete Properties Survey opened up a very wide range of problems concerning the effect of concrete properties on penetration. By altering the strength and composition of the target concrete, penetrations can be changed by a factor of three and one-half to one at 1000 feet per second and by a factor of five to one at 2000 feet per second. Obviously the effect of concrete properties cannot be neglected if any sort of quantitative predictions are to be made for concrete penetrations.

By early 1942, a new concrete laboratory and small-arms range were built at Princeton. M. E. DeReus was secured to take charge of concrete technology for the final Concrete Properties Survey. Caliber .50 model projectiles were used and two one-foot cube targets were made of each of

the 75 varieties of concrete tested for penetration. The tests continued through 1943 and the first part of 1944, and a parallel series of perforation tests were made on concrete slabs of various thicknesses, properties and reinforcing schemes. The results of all of these caliber .50 scale tests were reported in 1944. They constitute the best source of small-scale concrete penetration and perforation data obtained under well controlled laboratory conditions available to date.

In the same way, the results of the "Penetration and Explosion Tests on Concrete Slabs," mentioned above, and reported early in 1943, probably constitute the best source of large-scale data obtained under well controlled field conditions.

These "P&E" tests were conducted on 34 concrete targets, up to 30 feet by 28 feet by 76 inches in size, at the Aberdeen Proving Ground, using 37-mm., 75-mm., 3-inch and 155-mm. projectiles and striking velocities up to 3000 feet per second. Data were obtained on inert penetration and perforation, normal and oblique incidence, static explosions after inert penetration and live firing of HE projectiles.

By the time these experimental programs were completed, many other problems had become pressing for Division 2 and for the Corps of Engineers. Most of the personnel were transferred to other projects, but some further work on analysing the data, developing empirical formulas, and on the theory of penetration and perforation of concrete continued to the end of the war. Preliminary development work was also conducted on an electromagnetic method of obtaining an oscillographic record of the velocity of a projectile during penetration into concrete as a function of time. Measurements by this or some other method of the phenomena *during* penetration are now felt to be necessary in order to arrive at a satisfactory theory of penetration.

By the end of the war empirical formulas and methods had been developed to give satisfactory results for the design of fortifications and bomb shelters and for estimating the effects our own weapons can produce in attacking enemy concrete. Problems concerning projectile and bomb deformation against concrete, fuze design, fuze setting, and the design of composite targets (such as soil and concrete or concrete and steel) were less well understood. A theory, involving the force resisting the missile throughout the penetration cycle, has been proposed. This theory is in satisfactory agreement with the penetration observations, and may be used for the more complicated problems just mentioned. However, it cannot be considered proved at the present stage; other possibilities exist of accounting for the data. It is from this point of view that measurements during penetration are particularly needed.

In summary, it may be said that the knowledge of the terminal ballistics of concrete has been put on a firm experimental basis by the work conducted

during the war and described above. It is now in many ways as good as the corresponding knowledge on the terminal ballistics of steels and armor, whereas at the beginning of the war, it lagged far behind.

It is impossible to say whether, in case of another war, the studies will have to be continued or whether present information will be deemed sufficient. This would probably depend mainly on the nature of future weapons. It seems likely that higher striking velocities will become available, at least for projectiles if not for bombs. If so, some studies will be needed to extend the data to higher striking velocities than those for which data are now available.

## Shock Waves and Bombing Plans

If one did succeed in defeating direct hits, there might be damage from two other sources. A bomb which actually hit the target and did not perforate it would in almost all cases rebound and detonate in the air. It became necessary to study the effects of detonation in contact with the target and in the air nearby. This led to the measurement of transient phenomena in air resulting from the bomb explosion, especially the measurement of pressure as a function of time and distance from the explosion. A very large field of research was opened up as a result, having influence not only on the immediate problem of defense, but upon very much broader and more important problems.

For example, in the last few months of the war, the British dropped a few bombs which were probably at least three times as effective, weight for weight, as those used by them earlier. Another factor of nearly two should have been possible but was not put into use in time. This ratio of at least six may not be great compared with revolutionary advance of the atomic bomb but the enormous savings in Allied lives, airplanes, shipping and industrial production which could have been achieved had these advances been earlier utilized are revolutionary enough.

This large increase in efficiency was the result of four contributing causes: better explosives (using aluminum), larger bombs, lighter bomb cases, and air-burst fuzing.[8]

When a block-buster bomb exploded on a German city, the resulting destruction was largely caused by blast; that is, by the pressure waves or shock wave sent out by the explosion. It is therefore obvious that any study of the damaging power of bombs must include a study of these pressure waves and their properties. This was recognized from the beginning of Division 2 and steps were taken early to develop means of investigating these phenomena. The British had made notable advances in the early part of the war in devising electrical pressure gauges (using the piezoelectric

[8] The story of the struggle to develop better explosives — and to get them used after development — belongs for the most part to the history of Division 8.

property of quartz of generating a charge when compressed) which enabled the pressure in the blast wave to be recorded as a function of time. Samples of these gauges were brought back from Britain by Burchard in 1941 and work was begun at Princeton under the direction of Lampson to improve and utilize them for blast research. By the spring of 1942, a working outfit had been set up in the Princeton Ballistic Laboratory and has been used almost continuously since then.

It was evident, however, that portable equipment would be needed since only small charges could be fired at Princeton. Lampson and Bleakney, therefore, designed an elaborate Mobile Laboratory which was built under a subcontract by Herbach and Rademan of Philadelphia. This consisted of a truck with sixteen recording cathode ray oscillographs and the necessary auxiliary equipment for measuring all kinds of transient phenomena, not only air blast. Completed in the fall of 1942, it was almost immediately used at Aberdeen Proving Ground in connection with some tests of the effect of bombs on frame dwelling houses, which were being carried out by the Committee on Passive Protection Against Bombing and the Corps of Engineers.

Even before this test was completed, a duplicate mobile unit was designed for the use of the Ballistics Research Laboratory of the Aberdeen Proving Ground. This has been employed ever since (with many subsequent modifications) by the bomb group at Aberdeen.

Work was also begun in the spring of 1942 on air-blast gauges at Harvard under Wilson, then in Division 8. This work, which was eventually moved to Woods Hole and finally came under Division 2 in 1944, will not be treated here in any detail. It was at all times closely related to and coordinated with the work at Princeton. The emphasis was, however, somewhat more on the chemical side.

The development of suitable gauges and associated equipment proved to be a very formidable task and one which was not over at the end of the war. This was partly due to the fact that the requirements as to accuracy, range of pressures, range of charge size, etc., became continually more severe as new and more difficult problems were attacked. It was a never-ending problem for those responsible for the direction of the program to decide how much effort should be put into improving the measuring techniques and how much into securing useful results with the methods available, despite all their weaknesses.

The problem of gauge calibration proved equally difficult and led to the design by Reynolds and Taub of the "shock tube," a device in which a plane shock wave was produced by the bursting of a diaphragm separating two compartments in a pipe, one of which contained air under pressure. This was a modification of a similar device previously used by British investigators, Payman and Sheperd.

The shock tube was an effective instrument for calibration but its main usefulness was for the study of the Mach effect, a phenomenon which led to the idea of air burst. The story of air burst, used with such effectiveness with the atomic bomb, forms yet one more striking example of why revolutionary advances cannot be expected from research which is restricted to narrow practical ends. The idea that a bomb could be more effective when exploded at the right height above the ground than when ground burst grew out of the abstract mathematical work carried out by von Neumann for Division 8 on the theory of the interaction of shock waves. These studies were not instituted with any particular practical aim in mind but only with the scientists' faith that a deeper understanding of the nature of any given situation will in the end lead to more important practical results than a program restricted directly to short-range aims.

The most precise and clean-cut experimental study of shock wave reflection and interaction and of the special type of nonacoustic reflection called the Mach effect was carried out by L. P. Smith and D. K. Weimer at Princeton, using the shock tube. With this device, more than 2000 photographs were obtained showing the way in which shock waves of various strengths are reflected from a rigid surface at various angles. These precisely measurable photographs confirmed the basic theories of von Neumann.

A few observations on the effect of elevating charges above the ground were made by Taub and R. C. Stoner during an earth shock program at Camp Gruber (described later) but because of the pressure of the primary program using the Mobile Laboratory, not much could be done then.

The main investigations with actual explosive charges began in December 1943, and January 1944, at Woods Hole and Princeton. The Princeton program was laid out to provide a careful and systematic survey of the effect of the various variables such as charge height, gauge height and charge to gauge distance, especially as regards peak pressure. This was successfully carried through, under the direction of Reynolds and Stoner who followed plans made earlier by Taub. Charges of eight pounds were used at a location acquired by the Station near Princeton. The results obtained were co-ordinated with Smith's shock tube results to give considerable information about the peak pressure.

The work at Woods Hole, on the other hand, was planned with the much more limited objective of demonstrating the advantages of air burst and of locating within reasonable limits the optimum height. Great pressure was applied to get the results as quickly as possible and with the cooperation of the Stanolind Oil and Gas Company, which had built airblast equipment under Division 8, experiments were rapidly performed at scales of 2, 12 and 40 pounds and finally in May 1944, with 500-, 1000-, and 2000-pound bombs, the latter at the Jefferson Proving Ground in Indiana.

Just following the first Jefferson tests, a mission of bomb and explosive experts arrived in this country to attempt to persuade our Ordnance Department to use more modern explosives in the 4000-pound bombs being loaded in this country for the British. The British had, of course, been following the experiments on air burst in this country with interest but the opportunity of discussing the question personally with the members of the mission, as was done by Wilson and Taub, was probably helpful in getting the British to take up again their own work on this question.

The British had early had the notion that a bomb burst above ground might be more effective than a ground burst because there might thereby be less shielding by buildings. They performed experiments on a model town in 1941 to test this hypothesis and actually obtained results sufficiently encouraging so that operational trials were arranged. Unfortunately, at this early date (while Americans were still using paper gauges) the measuring techniques being developed by the British were not quite sufficiently advanced. The optimum height indicated by the model tests and used in the operational trials was too great so that disappointing results were obtained in the latter and air burst was dropped.

With the advent of von Neumann's theory and the supporting experiments at Princeton, Woods Hole and elsewhere, an entirely different reason for using air burst emerged; specifically, the nonacoustic reflection (Mach effect) from the ground redistributes the energy of the explosion in more favorable directions, less going straight up. The interest of the British was therefore rekindled and two sets of experiments were planned, which on completion towards the end of 1944, confirmed the Princeton and Woods Hole results.

In the meantime, efforts had been made to secure Service interest in this country. Division 4 had developed bomb fuzes suitable for airbursting bombs so that conferences were held by Wilson with the Chief of Division 4 and with Dr. R. D. Huntoon, also of Division 4.[9] These meetings followed the 12-pound series at Woods Hole and led to a conference with Brig. General R. C. Coupland of Air Ordnance. At this meeting, General Coupland expressed interest in the air burst work and requested to be informed concerning the 40-pound tests then being planned. Upon the completion of these tests another meeting with General Coupland took place which led to the Jefferson Proving Ground tests on a bomb scale. The arrangements for these were made by Colonel I. A. Luke and Colonel W. B. Hardigg of the Ordnance Department, at General Coupland's request. Colonel Hardigg provided every facility needed at Jefferson with the greatest promptness so that the Woods Hole group under Kennedy, by working quite literally night and day, were able to prepare the elaborate instrumentation needed and carry through the first series by May 6, 1944.

[9]Later with the Office of the Scientific Adviser of the Secretary of War.

At a meeting in Washington, the results of this series were communicated to General Coupland who then expressed a desire to have further tests using a different explosive. He arranged with Colonel R. G. Butler for a supply of bombs and authorization. These tests were carried out by June 2, 1944.

Previously Wilson had written to the Director, OSRD, and had pointed out the revolutionary possibilities of air burst. Dr. Bush replied and stated that he had brought the matter to the attention of the Joint New Weapons Committee of the Joint Chiefs of Staff.

Unfortunately little further interest seemed to be forthcoming from the Ordnance Department on this question. For one thing, air burst was technically limited to large bombs, which our air forces had always opposed, so that the airplanes then used by the AAF against Germany[10] could not carry 4000-pound bombs which the Ordnance Department had already produced (except under the wings, which had marked disadvantages). The B-29's which were beginning to be used against Japan, were, however, designed to carry them.

In December 1944, Wilson and Bleakney on the suggestion of Huntoon went to the Air Forces Board and discussed air burst with several of the board members, who were quite opposed to large bombs and therefore to air burst for blast purposes. However, the Board later issued a report favorable to this development.

Because of the widespread opposition to large bombs in the AAF and because only B-29's could carry them, it was logical for the Division to turn to the British, who were completely convinced of the superiority of large bombs for city and industrial targets. Therefore, Taub, on his trip to Great Britain in December 1944, made a special point of discussing air burst with various groups including the Static Detonation Committee and doubtless helped to clear away any remaining misconceptions. One of the factors which further stimulated British interest was the greater destructiveness of those German V-1 bombs which had struck the tops of trees and exploded above ground.

The British attempted to adapt the Division 4 fuze to their bombs but numerous differences of design made this a technically difficult problem which unfortunately was not completed before V–E Day.

In March 1945, a conference on air burst was arranged by Division 2 in Washington which had an excellent attendance from the Services.

Later, on March 16, 1945, a conference was arranged at which representatives of Division 2, of the Joint Target Group, of the Air Forces Scientific Advisory Group and of the Operational Analysis Division of the Air Forces agreed on a program of "controlled attacks," in which various types of weapons could be used under conditions such that a scientific evaluation

[10]B-17's and B-24's.

of their relative effectiveness would at least be possible. This program, in report form, was forwarded to the Twentieth Air Force through the Headquarters, Army Air Forces, and through the efforts of Dr. L. A. Brothers,[11] Chief, OAS, 20th Bomber Command a similar program of attacks was put into effect. Tests of the air burst principle were planned but V–J Day intervened.

Consequently, Hiroshima and Nagasaki were the only applications of the principle in practice.

The story of air burst has been given in so much detail because it is one more illustration of the failure of a sound and proved discovery to get into combat. Perhaps it is inevitable that over a year must pass before military interest in a scientific development can be aroused sufficiently to arrange for combat trials but it will always be hard to convince the scientists who worked with feverish effort and great enthusiasm that there is not something wrong with such a system.

## Big Explosion — No Earthquake

It was early evident that heavy fortification-type targets were not materially affected by detonations of small bombs in air, but that more serious effects might be experienced if the bombs penetrated the earth near-by and then went off. This led to a series of experiments with model and full-scale targets, including the measurement of the somewhat different transients experienced in the ground.

The statement of the problem by the Engineers was simple enough: what is the radius of damage to fortifications by explosions occurring in the ground? While there was some literature on the subject before the various Division 2 programs were undertaken, it was extremely spotty; by the time the Oklahoma Program sponsored by the Chief of Engineers, and carried out by the Tulsa District of the Corps of Engineers and the Committee on Fortification Design with the help of the Ordnance Department, Division 2, the Humble Oil Company and the Geophysical Research Corporation,[12] was completed and the data assimilated, a coherent series of relations between the variables involved had been determined.

The first step in the research program was the consultation of all discoverable sources, which turned out to be not very many. These writings were further remarkable for their circular character: each, in its various editions, would cite the other as authority, and it soon became obvious that serious investigation of the field still remained to be undertaken. Since plans for fortifications were then in the process of design, and the designs were being delayed until design information could be provided, it was not

[11] An alumnus of Division 2.

[12] Neither of which was an NDRC contractor. See Final Report of Committee on Fortification Design.

considered practical to start building up from fundamental aspects which were rather remote from the problem in hand. A series of empirical tests was first undertaken at Princeton at very small scale, to measure at least the relative effectiveness of various schemes intended to insulate fortification foundations from earth shock. During these tests, the only measuring technique that was successfully used was ordinary slow-motion cinematography (64 frames per second). Although these tests produced some results helpful to the Engineers, they were significant principally because they demonstrated the importance of variations in (1) the soil, both grain-size distribution and moisture content, (2) the design of the structural members receiving the sharpest loading, (3) the relation of structure to soil (intimacy of contact, depth of burial, etc.), and (4) the depth at which the explosive was detonated.

Two programs grew out of this first one: the more immediately important one was aimed at the development of instruments which would measure the behavior of the soil and the structure during the disturbance, and the other a further investigation of design criteria for fortifications. The instrumentation program was under the direction of Lampson; the various gauges and meters developed at Princeton were largely the result of the work of Reynolds.[13]

Although these two programs were designed and administered separately, there was considerable cross-fertilization, as the gauges helped define the engineering problems which then resolved themselves into smaller ones which demanded fresh measuring techniques. Finally the point was reached when it was felt that firm enough ground had been reached in both fields to warrant a thoroughgoing verification at full scale of all the bits-and-pieces tests that had been hitherto undertaken at a variety of small scales. A rather comprehensive program was therefore drawn up, comprising well over 100 separate tests. Since the most important determinations were to be those relating to scale, it was necessary to hold all the other variables constant. An area was selected where the soil was as homogeneous and representative as possible,[14] the number of types of fortification was held to a minimum, and all shot positions, gauge positions, and other dimensions of the layout were held to the predetermined scale factors.

This program involved the design and construction of various fortification components at various scales corresponding in size to the size of charge

[13]Assisted by Stoner in the development, and by Clarence Sassman, Ray Symons, and John Power in the construction. Some work on this program was carried out by Stanolind Oil and Gas Co., Humble Oil Co., and the Geophysical Research Corporation. Silverman was in charge of this work at Stanolind, Rust at Humble, and both Weatherby and H. T. Born at Geophysical Research. Wilbur, Newmark, and Martel (M.I.T., University of Illinois, and California Institute of Technology respectively) were the principal consultants on the structural program, in which the following members of the Princeton staff participated: Taub, White, Mayer, and Dahl.

[14]. . . and still so ununiform as to make correlations treacherous.

used.[15] While planning was going forward, increasing interest developed in the possibilities of attack on enemy installations by delay-fuzed large bombs which would penetrate the earth before detonation. This led to an addition to the program of another target and a 4000-pound bomb.

The site of this program was Camp Gruber, near Muskogee, Oklahoma, in the Tulsa District of the U.S. Army Engineers. Colonel F. J. Wilson was District Engineer, and through his good offices and his understanding of the program's needs, the direction by the Tulsa District was unerring. The program had been drawn up in consultation with the Office, Chief of Engineers, whose representatives, Lt. Colonel S. B. Smith and Christian Beck, made valuable contributions. The 20-odd tons of explosive to be used in these tests were provided by the Chief of Ordnance, including some experimental lots that had to be poured and boostered especially for this test program.

The instruments and mobile laboratories developed for the measurement of earth shock and its effects by the Princeton Station, by Geophysical Research Corporation, and by Humble Oil Company, were all brought into the field, and their operations integrated so that the most nearly complete picture possible could be got. Instruments of similar purpose were calibrated against each other to insure coherence of result data, and differences in tamping, detonating techniques and the like among the different groups were resolved. This standardization of instruments and techniques was of considerable help in explaining hitherto unaccountable discrepancies in the results of tests undertaken during the development period by each of the three groups.

The assimilation of the piles of records taken during this program led first of all to the formulation of the laws of propagation of motion in earth disturbed by impulsive loading. Some of the effects resulting from the presence of large and relatively elastic nonhomogeneities such as ledgerock and heavy fortification-type buildings were also determined. A working approximation of the loading on such structures was obtained empirically. Other empirical determinations were made concerning crater sizes and shapes, the significance of earth debris as lethal missiles, the effect of ground roll on brick walls on foundations of different depths, wave propagation through different types of soil, and numerous other points.

The Oklahoma Program results mark the deepest penetration yet made into matters concerning explosions underground. It was the culmination of the efforts in this direction made by Division 2, its subdivisions, and its subcontractors. Although this be the case, the investigations in this field have left much unexplored, and much that has been uncovered has been

[15] 1000-pound charge for full size (corresponding to 2000-pound bomb); 8-pound charge for 1/5 full size (scale of charge going with the cube of the linear dimension of the models) and various intermediate scales.

only superficial. What was done, however, served the desired purpose to some degree: where the Engineers had had to guess before, they now have at least some ideas as to orders of magnitude with which they have to contend (and in many cases information considerably more exact); where the Air Forces had had only pilot reports to tell them the effects of underground explosions, they now know better how to evaluate such reports, and they have definite data on which to base their choice of weapons; and more information of authority is now available to Ordnance to use as they choose. For most purposes of attack, the trials promised little for earth shock at least within the size of charges employed. The magnitudes, to be sure, were of dramatically lower order than those associated with atomic explosions; but for the magnitudes used the damage, severe if the charge were near-by, diminished rapidly as the charge was farther away; and the safe radius, being of the same order as the crater dimension, was in all cases one which made the probability of damage small within the precisions of aiming attainable by our Air Forces at the end of the war. There were no earthquakes or other similar horrendous phenomena.[16]

## Rapid Vibrations

In general, whether the attack was one of impact or of explosion, two effects had to be considered, one the local, the other the remote. That is to say, a projectile on hitting a target might make a hole through it or in it, completely destroying the close-by material. This was the local effect. The previous experiments had indicated how thick a target had to be to prevent perforation. There remained the question of whether the energy transmitted to the target by the impact might set up vibratory movement in a larger portion of the target, resulting in stresses beyond the capacity of the target. A similar impulsive load might be imposed by an earth or air shock. After a while, it became increasingly apparent that any target massive enough to resist the local effects would resist the remote effects. This was at best a very empirical conclusion, however, and work was undertaken

[16]One passes lightly over the difficulties of co-ordinating such an extensive field program. Cables get wet or kinked and short-circuit; power supplies break down; snakes wriggle into the field of action. Timing the event so that everyone is ready to record is always complicated. As in all matters of this sort communications break down. For example, the three field teams had to be tied together and each had to be connected to its shot crew. This was done by a network of intercom speakers, in addition to which the Geophysical crew used a field phone and Princeton a loudspeaker. This field set-up then had to be connected to the field house, a mile or so away. This was accomplished by an Army field phone. When the set-up was complete, and all wires had been checked, the first message was sent from the field house to the field crew. In the field house the Army phone was cranked. Finally the answering "hello" was heard, faintly but distinctly. Since the Army promised more volume on its phone, the field house man suggested that the field man get closer to the mouth piece. "There is no mouth piece here — I am 1500 feet from the phone. I heard your message over the loud speaker and I am answering you thru the field intercom!"

to determine the distribution of strain in various structural elements and combinations of elements subjected to extremely rapid loading.

Engineering design is usually concerned with structures, machines or other devices that must be strong enough not to come even close to failure as the result of the forces acting on them. In fact, the strength is generally made great enough so that the largest expected forces will not cause permanent distortion. Furthermore, practically all structures and a great many machines are subject only to forces that are essentially static, that is not necessarily forces that never change, but rather those that change slowly, if at all. Thus the weight of people on a floor is a static force on the floor. A weight falling from a certain height and striking the floor would exert a dynamic force.

Military structures or structures that may be subjected to attack of one kind or another cannot usually be designed to avoid all permanent distortion; nor should they be. The resistance of a slab or plate to the passage of a bullet or to the effect of blast, or the behavior of the side of a ship exposed to the shock from a mine or torpedo, are examples to which the usual engineering design principles are not applicable. In this situation it is impossible to make a design that will never show permanent distortion under any form of attack, and the object of the designer is simply to make the best possible structure consistent with the requirements of weight, cost, and availability of material with which he must comply. Conversely, the job of the attacker is to find the most economical means of destroying or damaging the structure that his enemy has built.

There are two important new questions that must be answered before either the designer of the structure or the man who is planning to attack it knows enough about the structure. These are: how does the structure behave when it is subjected to large distortions and what is the effect of deforming the structure very rapidly? There are two quite distinct effects resulting from rapidity of loading that must be investigated: the manner in which the rate of application of the load affects the intrinsic properties (strength, ductility) of the material of which a structure is built, and the way in which the effect of the load is propagated throughout a structure.

The first effect of the rapidity of loading, i.e. the effect on the intrinsic material properties, is illustrated by the behavior of a wire of ductile material such as iron or steel, to which a tension is applied. For slowly applied forces it is found that the wire has a definite elastic limit. A force smaller than this elastic limit will stretch the wire, but when released will allow the wire to assume its original length. A larger force will cause plastic deformation in the wire so that if the load is released the wire will be found to have elongated permanently. The force just necessary for plastic deformation is the elastic limit. For increasingly greater loads above the

elastic limit the permanent deformation increases, although the wire may not break. At a certain load, the ultimate strength, the wire breaks.

However, if the loads are applied very quickly and released immediately, it is found that forces much greater than the elastic limit may be applied without causing any permanent deformation, while forces greater than the ultimate load may be applied without causing rupture.

The propagation effect is due to the fact that the effect of a sudden force is not transmitted instantaneously to all parts of a structure. Instead, the portions nearest the point of application first experience the effect, then those farther and farther away. The time required for the effect to be transmitted is very short, since it travels with a very large velocity, of the order of 1000 to 20,000 feet per second in most structural materials. For forces that are applied slowly the propagation effect is unimportant because by the time the application is complete the entire structure has felt the effect. As the force is applied more and more rapidly its effect tends to be more and more concentrated near the point of application. Finally, the structure may break at the load without the rest of the structure being affected at all.

This is illustrated by the behavior of a ductile wire held firmly at one end and pulled at the other. For low pulling speeds the wire first stretches uniformly and finally breaks. The break may occur anywhere, depending usually on the presence of a local flaw or other weakness. However, for a certain range of speeds of pulling, the wire will tend to break at the held end (not the pulled end), provided there are no sections weak enough to overcome this tendency. The entire wire will, however, be stretched before the break occurs. Furthermore, for pulling speeds above a particular speed, the critical velocity (depending on the material), the wire will tend to break at the pulled end, with little or no stretching elsewhere. Since the amount of energy required to break such a wire increases with the amount of plastic deformation produced in the entire wire before breaking, it is found that much less energy is required at speeds above the critical velocity than below. Similar effects exist for more complex structures.

An extensive series of tests was carried out in Division 2 on various phases of dynamic loading. Accompanying these experiments were theoretical treatments aimed at explaining or interpreting the phenomena. Four series of tests were planned for measuring the change in intrinsic material properties with changing rate of loading. At the Massachusetts Institute of Technology the behavior of steel and copper specimens of cylindrical shape at very high speeds of stretching were studied, and the forces measured, by de Forest. The apparatus used consisted of two machined steel sections in contact, with a hollow space containing gunpowder between them. One end of the specimen was attached to each steel section and was stretched

and broken by explosion of the powder. The force required for stretching was measured electronically.

Another series of tests was carried out at the University of Pennsylvania and the Carnegie Institute of Technology under the direction of Seitz (now of Carnegie).[17] In this series of experiments short steel and copper specimens were compressed very rapidly and the necessary forces measured and compared with those required for slow compression. A rotary testing machine was used. This machine consists of a heavy flywheel on a horizontal axle that can be rotated at any desired speed within a certain range. The specimen to be tested is placed in front of and very close to the rim of the wheel on the level of the axle. At the proper instant, a pair of horns are caused to project suddenly from the wheel, striking and compressing the specimen. The specimen rests on an electronic gauge by means of which the compression forces are measured.

Somewhat similar wheels were used to break tensile specimens in two series of tests at the Westinghouse Research Laboratories and the California Institute of Technology, respectively. These tests were made on a large variety of steels, aluminum and zinc alloys, and copper.

The Westinghouse tests were carried out by J. Miklowitz under the direction of Nadai, who was a pioneer in this field of material testing and is recognized as one of the leading authorities on the plasticity of metals.

An additional series of tests at the California Institute of Technology was concerned with the propagation effect. In these tests a guillotine-type testing machine, consisting of a pair of steel rails between which slides a weight attached to heavy rubber bands, was used. The specimens were mostly long wires of various metals tested in tension although compression specimens and laterally loaded beams and plates were also tested. Both series of California Institute of Technology tests were under the over-all direction of Professor Clark.[18]

An important supplement to the impact tests for strain-rate and propagation effects was their analysis by mathematical methods. A theory of plastic wave propagation was devised and extended to cover most of the experiments performed.[19]

Impact tests on reinforced concrete beams were made at the University

[17]Assisting him were Drs. A. W. Lawson and P. H. Miller (Pennsylvania), and Drs. O. C. Simpson, W. H. Bessey, G. H. Winslow and R. J. Eichelberger (Carnegie).

[18]Dr. Pol Duwez, and Messrs. D. S. Wood, H. H. Madley and H. E. Martens planned and carried out the research, based on theories first published by von Karman. This and the related work was transferred to Division 18 late in the war when it became apparent that its applications to the war problems of Division 2 would be too late to be useful.

[19]This was the work of von Karman (California Institute of Technology) and Bohnenblust (Princeton), and Drs. J. Koehler (Carnegie), D. H. Hyers, J. V. Charyk (California Institute of Technology), and White (Princeton). Some of the theoretical development was the work of individuals outside of the division, especially Professor G. I. Taylor (Cambridge University, England).

of Illinois in order to determine the ultimate resistance of such structural units to impulsive loads, and the best design of such units. This work was directed by Richart and Newmark.[20]

The analysis and design of structures subjected to impulsive loads was studied at the Massachusetts Institute of Technology and at Princeton University at the request of the Army Corps of Engineers. Rational methods of design were devised. The M.I.T. project was directed by Wilbur.[21]

The deformation of steel under very high pressures, such as those occurring during the penetration of armor steel by projectiles, was investigated at Harvard University by Bridgman.[22]

Work of this sort requires later correlation and application before it can be said to have a serious effect upon the design of a fortification, a ship or other structure subjected to impulsive loading. Since early tests had suggested empirically that resistance to the local effect would almost certainly insure resistance to the total effect, the utility of this fundamental information would come in determining whether, having selected a proper resistance to a local effect, there was a most economical way of deploying the material throughout the structure (that is, would there be an optimum span for a fortification, etc.). The Division was frequently called into consultation on the important problem of impulsive loads which concerned those who were worried about the damage to ships' hulls from underwater explosions, the minimizing of harm to personnel from crash landings or aircraft, and many other subjects of immediate importance. To such problems the Divisional experience could lend some assistance, but it was primarily focused on fundamentals and in this field the establishment of the fundamental knowledge and the application to an actual target inevitably consumes time.[23] Accordingly the principal achievements in this area were in the advancement of knowledge for later use rather than in immediate applications to combat vehicles, ships or forts.

## For the Merchantmen at Sea

Interest in the ballistics of projectiles and some scattered work on the conventional methods of armor testing naturally created a store of information and a resource of equipment which could be put to other useful purposes. One of the more significant of these came when a representative of the British Admiralty asked the Division to undertake studies on how plastic armor performed. This material was an interesting development of

[20]Dr. R. B. Peck and Messrs. H. C. Roberts, C. F. Shriver, W. H. Munse, T. L. Speer and W. L. Ogden were principally concerned.

[21]Assisted by Professors W. M. Fife and C. H. Norris, Dr. C. F. Peck, and M. M. Platt.

[22]Assisted by C. Lanza. The special apparatus required in this work was constructed by Charles Chase.

[23]Consider for example how long a time ensues between the beginning of the paper designs of a battleship and the end of the ship's shake-down cruise.

the war, a special concrete made of stone aggregate and bitumen mixed with a filler such as limestone dust and usually backed by a thin plate of mild steel; this was devised, when conventional armor was scarce, as a method of protecting some parts of merchant vessels, landing craft and the like against machine-gun strafing. It had good resistance to projectiles of small caliber and to high-velocity fragments, could be formed readily in the curious shapes required on shipboard, and when holed could be easily repaired, since all that was required was some tar, some stone and some heat. Conventional methods of armor testing proved utterly inadequate as a basis for comparing various compositions and a study of better ways led to some clever deductions and interesting results.

Soon after the usefulness of such armor had been demonstrated, the Americans adopted a substantially identical material called "plastic protection" for use particularly on merchant ships, and undertook the manufacture of large quantities of this material.

Considerable work was done in England and later in the United States to determine how good the material was for stopping bullets of various types as compared say with an equal weight of steel, and to answer a large number of questions such as: what types and sizes of stone were most effective; and how the stopping power varied with composition and thickness of the "plastic" layer and of the backing plate.

It was at once evident to most people interested in this work that the ballistic merit of a given type of plastic armor or plastic protection with respect to a particular projectile could not be specified as readily as it could for a homogeneous material like steel. The ballistic merit of a steel plate or concrete slab may be specified in terms of a limit velocity determined by firing pairs of shots with slightly different velocities and adjusting the velocities until the shot with the lower velocity does not perforate the material while the one with the slightly higher velocity goes through. Though occasionally workers in England attempted to apply this testing procedure to plastic armor, the more usual procedure was to fire a number of shots of given velocity at slabs of a given type and to rate the material either simply according to the fraction of bullets that perforated or in accordance with the "British Index." This index was a mean of numbers assigned to each shot according to the degree of penetration the bullet achieved. Its use represented an attempt to arrive at a figure of merit from a given number of shots based on more information and hence presumably more reliable than the figure representing "per cent through."

Toward the end of 1942, Lt. Comdr. A. H. Laurie, R. N. V. R., a representative of the British Admiralty in the United States, became interested in having experiments performed at the Princeton University Station to learn about the mechanisms by which plastic protection stops bullets. Such information might lead to better ways of rating the material or to sug-

gestions as to how the performance could be improved. The Bureau of Ships of the U.S. Navy Department was also interested in the possibilities of obtaining such information and at their suggestion a project to study plastic protection was started at the Princeton Station in January, 1943.

The work at Princeton was initially under the direction of Beth, assisted by Pittenger. A number of slabs furnished by the Flintkote Company were shot with caliber .30 A.P. M-2 projectiles and the damage done to the slabs and to the cores of the projectiles was studied. It was found that identical projectiles, similarly fired, behave very differently, as one might expect because of the inhomogeneous nature of the stone-bitumen mixture, and that fragments of projectile cores embedded in the slabs ranged in size from dust particles to undamaged cores. Flash photographs of bullets as they entered the front surfaces of slabs were obtained and a few X-ray photographs were made at Frankford Arsenal showing core fragments embedded in slabs. All these experiments indicated that the chief mechanism of stopping projectiles was the turning of many A.P. cores or their shattering by the stones of the plastic layer so that if they reached the backing plate they could pass through it much less readily than undeflected, intact cores of the same speed. The experiments also demonstrated quite conclusively that any detailed theory of the behavior of a bullet as it traversed a slab of plastic protection would be hopelessly complex.

When direction of the plastic-protection project was taken over in March 1943, by L. G. Smith, it was decided that the Princeton Station could contribute most to the studies of plastic protection by investigating the statistics of test results with a view to obtaining estimates of the reliability of such results as a function of the number of similar shots used, or conversely, to specifying the number of shots required in future tests to obtain results of a given, desired reliability. Preliminary investigation revealed that the reliability which could be attached to a test involving as few as 10 or even 20 shots similarly fired at similar slabs was far less than most workers in the field appeared to realize. Thus, for example, it was found that if only 10 shots are fired similarly at a given slab and half of them penetrate while the rest are stopped, one can say only that the true probability of perforation lies between 18 per cent and 82 per cent if one wishes to have 95 per cent chance of being correct. If 50 out of 100 shots were found to perforate, these limits would be 40 and 60 per cent.

The need for more extensive tests, involving more than the then customary 10 (or at most 20) shots of a given sort against a particular type of slab, was recognized independently by the Bureau of Ships which sponsored in the spring of 1943 a very extensive series of tests carried out by the Flintkote Company of caliber .30 A.P. M-2 projectiles with four different striking velocities fired into slabs of plastic protection of thicknesses varying from 1 to 2½ inches. For each striking velocity and slab thickness be-

tween 90 and 130 shots were fired. These firing tests were supplemented by similar ones performed at the Princeton Station by Pittenger at a velocity above the muzzle velocity of Service ammunition and at one lower velocity. The velocities of sample projectiles from each lot used by the Flintkote Company were measured with a chronograph at the Princeton Station.

An extensive analysis of the results of all these firing tests by statistical methods was reported by Smith. Curves of this report showed strikingly the difference between the ballistic behavior of plastic protection and that of a material like steel. Thus for a given thickness of plastic protection the probability of perforation rises relatively slowly as velocity is increased while for steel it rises quite abruptly from 0 to 1 as the "limit velocity" is passed. The curves showed clearly that the concept of limit velocity is inapplicable to plastic protection. They also showed that this material is relatively superior to steel armor for defense against attack by caliber .30 A.P. M-2 bullets at normal incidence at high striking velocities (because it stops some bullets while steel stops none) and is inferior at low velocities (because it stops only some while steel stops all).

When the work on plastic protection was terminated, early in 1944, the behavior of the material when attacked at normal incidence by caliber .30 A.P. M-2 bullets at ranges up to 600 yards was known with satisfactory precision. Questions which remained to be answered concerned its behavior at oblique incidence and when struck with other projectiles, particularly caliber .50 projectiles and bomb fragments. The slow rise of the perforation probability with striking velocity for caliber .30 bullets as well as a few scattered experiments indicated that the material might be considerably superior to equal weights of steel in protection against bomb fragments, most of which have high velocities. Also much of the early test work, where attempts had been made to establish the best types and proportions of materials to be used, was shown by the statistical considerations to be of questionable value because of the small number of shots which had been relied upon, and probably should be repeated. From this work it was possible to establish criteria for acceptance and for determining which combinations were in effect the best to take into combat either in the battle of supply or the battle of the beaches.

## Defense Against the Hollow Charge

A new weapon of this war was the shaped charge. Many years before the war, an American named Munroe had shown that, when explosive charges were hollowed out in the side facing the target, frequently more damage could be done than with a solid charge.

While it had thus been known for many years that explosive charges containing cavities produce deeper holes in steel than these same charges without cavities, the fact that a charge whose cavity was lined with thin

metal or other materials would produce much deeper holes does not appear to have been discovered until the beginning of this war. Charges having cavities lined with metals were found to be capable of perforating very large thicknesses of armor plate or reinforced concrete. This surprising fact appears to have been discovered almost simultaneously in many parts of the world, since weapons[24] employing this principle soon appeared with the armed forces of all the nations involved. At the outset researches were started with the purpose of improving the weapons.

In September 1942 these researches had progressed so far that a joint Army-Navy-NDRC committee on shaped charges decided that it was time to investigate countermeasures. A project was initiated in that month and it was suggested that plastic armor be the material with which to begin the work. Some delay was encountered in finding a contractor but early in 1943 work was started at the Carnegie Institute of Technology,[25] which had available personnel and which was near to the Explosives Research Laboratory, Bruceton, Pennsylvania, which was successfully investigating the principles needed for weapon development.[26]

So little was known about the phenomenon at the start that an intensive program of investigating fundamental principles was started along with a vigorous search for a practical protective material. One such material had been suggested at the start.[27] Later developments in the theory of protection showed that this material had been an excellent choice for several reasons. The Flintkote Company of New Jersey, acting as subcontractor, manufactured the material and co-operated throughout on many phases of the project.[28]

Since the Sherman tank was the one most used by our forces, most at-

[24] The most effective of these weapons were the American and German Bazookas and the German Panzerfaust, all of which were designed as antitank weapons to be carried and operated by individual infantrymen. The Bazookas employed the rocket principle for getting the lined cavity charge to the tank; while the Panzerfaust employed a very light recoilless mortar for this purpose. Other methods employed for transporting cavity charges to their targets varied from shooting them in artillery shells to attaching them by hand with permanent magnets.

[25] The work at Carnegie was directed by Pugh, with Seitz and Dr. Otto Stern acting as consultants. Dr. Turner L. Smith and Robert Lew contributed much to the early part of the project before they were called to other high-priority research. Robert J. Eichelberger was in charge of the work on tank protection. He was aided in this by Dr. George H. Winslow, who also took charge of some work on protection of concrete fortifications against these charges. Robert von Heine-Geldern assisted in both these programs.

[26] This work was under Division 8, NDRC. Dr. G. B. Kistiakowsky, then Chief of Division 8 and a member of the Joint Committee on Shaped Charges, transmitted the request of that Committee that Division 2 undertake some aspects of the study of the converse problem.

[27] The material, a combination with "plastic armor," had already engaged the attention of the division in another context (see p. 272).

[28] P. R. Smith and George Cawley of this company were particularly helpful in designing and constructing devices for fastening protective material to existing tanks.

tention was devoted to the protection of this vehicle. Two practical devices using two entirely different principles were developed for this purpose. These were tested against all manner of American weapons, including armor-piercing and ordinary high-explosive projectiles. They were also tested against German shaped-charge weapons as soon as these became available.

Although the potential threat of bazooka-like shaped-charge weapons was very great, they were not an important menace until the closing phase of the European campaign and were never serious. This was largely due to the fact that the Germans concentrated most of their efforts on their Panzerfaust, which was so large that it could perforate 8 or 10 inches of armor plate, but largely because of its size and its low velocity was so inaccurate that it seldom hit its target. The German choice made adequate protection very heavy and at the same time reduced the need for this protection. Consequently neither type of protection was used in combat. However, protection had been developed which could have been used had the Germans made the last-ditch stand with the great quantities of improved Panzerfausts that had been issued to soldiers and civilians for this purpose.

A rather complete theory of the penetration by shaped-charge jets was developed and tested.[29] This theory made it possible to determine the kinds of materials that would make good protection. It showed that for the vast majority of materials, including armor plate, density was the only physical property that need be considered. From this theory the optimum density for a practical protective material was determined.[30]

## Will High-Speed Butter Beat High-Grade Armor?

As the Division commenced to master the defensive techniques, it became evident that much of its knowledge would be useful both in planning new weapons for the attack and even the attack itself. For example, the penetration studies could be immediately applied to determine whether our bombs and projectiles would defeat enemy installations; the work on measurement of transients could be used to find the probable effectiveness of various explosives and combinations of explosives and the best sizes of charges to develop; the work on earth shock could be applied to examine the merit of attacking an otherwise unassailable target by indirection.

More direct studies of offense grew in importance. The rather routine early work at Princeton University on armor penetration had developed equipment, techniques and personnel admirably fitted to study more exciting problems. Again it was natural that the terminal ballistics of hyper-

[29] The quantitative form of the theory was the work of Dr. Pugh and his colleagues and was based largely on the experiments and qualitative generalizations made by Dr. D. P. MacDougall and his colleagues at the Division 8 Laboratory at Bruceton.

[30] The mathematical analysis in the theory was carried out by Edward L. Fireman. Drs. W. H. Bessey and A. G. Strandhagen acted as consultants in this part of the work.

velocity should be assigned to Division 2. Before the war, a German, Gerlich, had proposed that the barrel of a gun be tapered inside so that the pressure would act over a greater area during acceleration and create a substantially greater muzzle velocity than was otherwise possible. This had been published in American ordnance journals. When Gerlich guns appeared in Africa with Rommel's troops and when their projectiles were rumored to be piercing our tanks with alarming regularity, our side became desperately interested in supervelocities and intensified its research. (Where the ordinary velocity of a projectile seldom exceeded 2600 feet per second at the muzzle, hypervelocities of 4000 feet per second or more were reached. Since the energy of a projectile is a function of the square of the velocity this meant that the energy of the hypervelocity projectiles might be 2½ or more times that of ordinary projectiles of equal mass). Division 1 had been entrusted early with the problem of developing ways of producing this velocity for combat weapons, there being several other approaches besides that of the tapered bore, which is hard to make in quantity. It did not necessarily follow, however, that this supervelocity would prove useful. The projectile might, for instance, shatter on impact at such high velocities. To Division 2, then, was assigned the problem of working out what did happen when such rapidly moving objects met armor.

There were two distinct phases to this work. The early work reflected the general concern of the country, before and at the beginning of the war, over defense; at this time interest centered mainly in the properties of the plate and its ability to resist perforation. During this initial stage tests were made only at the relatively low velocities that could be obtained with conventional guns and projectiles. In the later work — beginning in August 1942 — the experimental range of velocities was extended from 3000 feet per second to 5500 feet per second and the emphasis shifted from a study of plate behavior to consideration of projectile performance, particularly as it was affected by deformations. Despite this change in emphasis, however, the program continued, as originally planned, to be an empirical study of the general problem of projectile impacts against armor over the complete range of practical velocities. Although the present section is concerned only with those aspects of the program dealing with armor-piercing projectiles, the group headed by Curtis,[31] and charged with carrying out the work, dealt with other problems as well.[32]

In the beginning it was necessary not only to train men who had had no

[31] The group dealing directly with armor studies was headed by Curtis. Experimental work was supervised by R. J. Emrich and R. L. Kramer. Messrs. C. H. Fletcher, Ross Thackery, J. R. Sproule, Henry Davis and L. I. Shipman contributed to the development of useful pieces of equipment. Almost every member of the Princeton University Station helped in the work at one time or another. Robertson, Bleakney and Taub were particularly active during the early stages.

[32] Notable among these was the problem of the "frangible projectile" which was developed for the training of flexible gunners. See Chapter XXX.

previous experience in the field of terminal ballistics but also to build apparatus and develop techniques for making measurements. The history of the project was similar in this respect to that of all general research programs; a preliminary preparation period of several months was required before significant results were obtained. Time spent at the start in the development of new and better techniques of measurement paid dividends throughout all the later stages of the work, however. Notable among these developments were three pieces of equipment for measuring projectile velocities: (a) a precision double ballistic pendulum, (b) a spiral chronograph and light screens, and (c) a double-spark unit. All had the advantage that they not only measured velocities with high accuracy but gave essentially instantaneous values at a particular point rather than average values over a long distance. The necessity of correcting for losses due to air resistance was avoided and a long range was not required for firing trials. In addition to measuring velocity the double-spark unit furnished a record, in the form of spark shadowgraphs, of the deformation suffered by the projectile as a result of the impact.

Immediately following the development of measuring equipment, tests were carried out to determine how various properties of armor plate affected its ability to resist perforation. Since a plate is most vulnerable under attack by nondeforming projectiles at normal incidence, firings were carried out under these conditions. Use of a nondeforming projectile had the further advantage that the energy required for perforation was dependent only on the properties of the plate and the projectile's shape and mass. Trials indicated that the principal factors controlling the energy needed to defeat a plate under these conditions of impact were its hardness and thickness and the size of the projectile. Data were obtained to show the effect of changes in each of these variables. Changes in mass and nose shape of the projectile proved to be relatively unimportant although they did have a measurable effect; a small-scale effect appeared in the sense that with similar plates and projectiles a larger projectile could perforate at a lower velocity. It was also shown that the lateral dimensions of a plate and its method of support were of secondary importance. The last of these observations implied that no form of springs, cushions or nets could add substantially to the stopping power of the target and so ruled out such proposals as that of covering the Panama Canal with a net of steel cables to protect it against bombs.

Even before the tests mentioned above were completed others were begun to determine the feasibility of using hypervelocity projectiles as a means of perforating thicker armor. Interest in unconventional methods of gun and projectile design was aroused because of a report from the Libyan campaign in North Africa that the Germans were using a hypervelocity tapered-bore gun as an antitank weapon. Actually this gun was not extensively

used by the enemy but it did lead to a momentary setback in our confidence and consequently provided a spur to programs for the development of hypervelocity weapons in the United States, England and Canada.[33] Before the end of the war all countries had successfully employed in combat high-velocity, tungsten-carbide-cored projectiles which were provided for guns of various calibers. The principal Service type of the Americans and of the Germans was a composite rigid; that of the British was the discarding sabot.[34] The development of other methods had been brought to a successful conclusion but these methods were not widely used in the theaters of operation.

The need for a program to investigate the factors affecting the terminal ballistic performance of hypervelocity projectiles was emphasized by another report from the Libyan campaign. It appeared that guns firing 2-pounder A.P. shot were much less effective at point-blank than at long range, thus indicating that a projectile might become less rather than more effective when its velocity is increased. Although the British had accepted the validity of this operational report, considerable skepticism was expressed in this country. To resolve the question whether a projectile could completely disintegrate and fail to perforate a plate at a high velocity, and yet remain intact and succeed at a lower velocity, an extensive series of firings was carried out which not only showed that such a result was possible but indicated the conditions under which it would occur.

Similar to the disbelief in the possible existence of a "shatter gap," in which a projectile can perforate a plate at velocities above and below but not within a given range, was the misconception that the strength of a projectile material was of little consequence at high velocities. Thus a well-known ordnance book states that, "if very high striking velocities are obtained, penetration is little affected by the material of which the projectile is composed." It is conceivable that this might be true at sufficiently high velocities, but it proved not to be true up to 5000 feet per second, which is higher than the velocity of any present-day projectile in practical use.

In order to answer the above and similar questions on the effects of projectile deformations, an extensive series of tests was performed using a caliber .50 arrowhead projectile containing a caliber .30 A.P. M-2 steel core. Velocities up to 5400 feet per second were obtained. It was shown that deformation of the projectile increased progressively with striking velocity over a range of several hundred feet per second. At the top of this range where the nose completely disintegrated — at the shatter velocity — the energy absorbed by the plate suddenly increased tremendously for all but very large angles of attack. This abrupt increase in the energy absorbed placed a practical upper limit on velocity for a given projectile.

[33]Carried on in America largely by Division 1.
[34]See Part Three.

Since the preliminary results had indicated that shatter of the projectile was the most serious limitation to the use of hypervelocities, all subsequent trials were conducted either to test means of raising the shatter velocity or to determine causes of projectile deformation. In the first of these later trials, caps which had been frequently used with standard projectiles were tried on steel hypervelocity projectiles. The results cannot be completely summarized by a brief statement. Suffice it to say that the cap was found extremely effective in preventing deformation of the projectile proper and led to an improvement in perforating ability under most but not under all conditions of attack. At very high angles of incidence (greater than 50°), for example, a projectile with a cap will ricochet and be less effective against thin plate than a monobloc projectile that shatters. This apparently anomalous result explains the fact that monobloc types were better than capped projectiles for defeating the sloping plates on the front of German tanks.

Despite certain sacrifices the cap had to be used if the projectile were to be fired at hypervelocities. On the basis of results obtained with a capped projectile whose core stayed intact against homogeneous armor at all obliquities and all velocities up to 4000 feet per second, it was shown that 30 per cent thicker plate could be perforated with a subcaliber steel projectile than with a similar full caliber projectile fired from the same gun.[35] This figure applies at the muzzle of a 37-mm. gun, which was chosen as an example. The subprojectile should perforate 20 per cent more armor at 2000 yards.

It was clear from other tests that the 30 per cent advantage possible with a subcaliber steel projectile would not compare with results that can be obtained from the use of a tungsten carbide core. The advantage of tungsten carbide, which occurs mainly because of its high density, may easily be as much as 60 per cent if the projectile is properly designed. Without proper design, however, results very similar to shatter in the case of steel projectiles will prevent attainment of the full benefits to be expected.[36]

This point was amply demonstrated during a program under the dual sponsorship of Divisions 1 and 2. Division 2 had been asked by Division 1 to assist in improving the terminal ballistic performance of a 57/40 mm. folding skirt projectile which was being developed by the Jones and Lamson Machine Co. under Division 1 and which had previously shown poor performance against armor in a preliminary test at the Aberdeen Proving Ground. As part of the subsequent development program, a comparative plating trial was carried out with the original model of the 57/40 mm. and

[35] In this comparison every advantage was given to the subcaliber projectile so that 30 per cent really represents an upper limit which could probably not be obtained in practice.

[36] The division was barred from design by the Ordnance Department. See Chapter XXIX, p. 320.

with the British 6-pounder D.S. MK I. The principal difference between these two projectiles was that the 6-pounder had a heavy steel windshield and duralumin pad while the 57/40 mm. had only a thin duralumin windshield. Because of the added protection to the nose of the tungsten carbide core the 6-pounder was able to perforate considerably more armor for all but near-normal obliquities. This advantage of the 6-pounder increased as the striking velocity of the two projectiles was raised, being greatest at 4150 feet per second, which was the highest velocity used. By modifying the carrier design of the 57/40 mm., performance was obtained which was equally as good as that of the 6-pounder under all and better under certain conditions of attack. At the muzzle velocity of the gun and at a 45° angle of attack the original model of the 57/40 mm. failed to perforate 4-inch armor, whereas the modified design perforated 6-inch plate.

During the latter part of the work by the division, all experiments were performed with tungsten carbide projectiles. An extensive program was set up systematically to test the effect of changes in each of the projectile parameters. Only one of the results of this program will be discussed here. This example is chosen because it illustrates the need for studies designed to obtain an understanding of the phenomena involved rather than to determine immediate practical answers directly.[37]

Previous to the tests mentioned above a tremendous amount of time, money and materials had been spent in tests to determine the best composition for a tungsten carbide core. All trials had been carried out, however, by firing projectiles with different compositions under very special and not well chosen conditions of attack. For example, in one case perforation limits were determined by firing a projectile with a 1.5-inch-diameter core against 4-inch homogeneous armor at 30°. Neither this nor any other test had shown a significant difference in the energy required for perforation due to a change in composition, that is, due to a change in the mechanical strength of the material. The reasons for this were clear when a few tests were performed at Princeton to determine the shatter characteristics of tungsten carbide. It was evident that all previous tests had been carried out under conditions such that (a) either none of the projectiles would shatter regardless of their composition, (b) or all projectiles would

[37]Such a program with so many implications should obviously have been initiated at least two years earlier but was not because the Ordnance Department contended that they would never have need for projectiles of this type and consequently did not support development projects. Although later the Ordnance Department professed great interest in the results, the true extent of this interest is perhaps manifested by the fact that at a critical time in the manpower situation they failed to certify this project as one for which draft deferment would be justified. In theory this would have required the transfer of all draft-age workers to projects which had been so certified. In practice the division was able to arrange to keep on working on the project, although not as efficiently as if it had had the support from Ordnance which the division thought it deserved and which later events proved would have been desirable.

shatter, (c) or the breakup of the projectile was not important in controlling the limit. In none of these cases would a real difference in the limit energy be expected; such a difference should occur only when the striking velocity was between the shatter velocities of the projectiles being compared. Under these conditions, however, the difference in performance should be great. With only a relatively few shots it was possible to establish these facts and set up a rational basis for the choice of composition.

Before the program on tungsten carbide projectiles was completed the work was terminated by the end of the war. The practicality of the use of hypervelocity tungsten-carbide-cored projectiles had been established, however, both by "plating" trials and, as has been previously mentioned, by actual combat use. The most recent estimate of the performance of American and British antitank guns against German tanks indicates that in the case of every gun, sabot or composite rigid projectiles are more effective in perforating the protective armor than the corresponding standard full-caliber steel projectiles.

This brief history has touched only on certain aspects of the work done on the terminal ballistics of armor by Division 2. The general problem undertaken was that of determining how the energy required to defeat a steel target depends on the mass, size, shape and mechanical strength of the projectile components as well as the hardness, thickness and arrangement of the plates composing the target. The problem was complicated not only by the large number of variables involved but by the likely occurrence of projectile deformation. It was impossible to consider all types of targets and to investigate all variables in projectile design. A considerable amount of work remains to be done before a completely rational system of projectile design can be devised.

## No Rapid Fire in a Dust Cloud

The knowledge of transient phenomena in air proved unexpectedly useful in the attack on an urgent gun problem. Medium-caliber guns that had been developed for use against aircraft were adapted during the war for use in direct fire against armored vehicles and pillboxes. Such guns used fixed rounds with muzzle velocities of from 2600 to 3000 feet per second; the rate of fire for 10 to 20 rounds was limited only by the speed with which the gun crew could reload after the automatic ejection of the shell case. The weight of the charge in these rounds was about one-third that of the projectile, or about five pounds in the three-inch gun. Such a charge produced high pressures, the average pressure in the gun tube being 10,000 pounds per square inch or greater at the time the projectile left the muzzle; this resulted in a severe muzzle blast. When fired at low elevations, the guns produced serious blast effects, one of the most persistent and annoying of which was the obscuration of the target caused by the raising

of dust. A high rate of fire is useless, of course, when the gun position is enveloped in a cloud of dust, for in direct fire the target must be seen and, when the target is a moving vehicle, the strike of the projectile must be sensed by the gunner.

As the war progressed the trend continued toward the development of higher muzzle velocities and rates of fire, but it soon became apparent that muzzle blast effects limited the tactical use of these weapons. Among the proposals suggested to relieve obscuration from dust were the use of mats to cover the ground near the muzzle, the chemical treatment of the soil in front of the gun position, and the use of muzzle attachments to deflect the blast from the ground or otherwise render it innocuous by destroying its energy. The first two methods required the transportation of relatively heavy equipment and time for the preparation of the gun position. This was disadvantageous where high mobility was required. The solution of the problem through the use of muzzle attachment was certainly the most attractive.

The only muzzle attachments that were used on medium-caliber guns were muzzle brakes. These were first used extensively by the Germans and later adopted to some extent by the armies of other nations. In the American Army there was at first a prejudice against the use of muzzle brakes perhaps because the function of these attachments was not fully understood; but our gun crews began to demand them when they saw brakes being used on the German guns. The only brake our Army put into the field in moderate quantities was the M-2 on some of the later 76-mm. guns mounted on tanks and tank destroyers. This brake was a "streamlined" version of the standard two-baffle German brake, the modifications consisting principally in changes in the proportions, some of which appeared to reduce the efficiency of the unit. A more reasoned demand for the development of muzzle attachments began when it was noticed that under certain conditions the brake of standard type greatly reduced obscuration from dust. In fact, it was this property as much as any other feature of the brake that led to its standardization, for it was not needed, as a brake, on any of the vehicles on which it was used in the field.

At the request of the Ground Forces,[38] Division 2 agreed to attempt the development of a muzzle attachment that would suppress the raising of dust. Contracts for this work were given to the Princeton University Station,[39] the General Electric Company,[40] and the California Institute of

[38]Whose eager support throughout the project made possible whatever results were achieved.

[39]The work at Princeton was under the direction of Slade, who was assisted by Kramer and Fletcher. Several others of the Princeton staff deserve mention for their assistance on this project. They include Taub, Reynolds, Curtis and Bleakney.

[40]Under the general direction of E. L. Robinson, Structural Engineer, Turbine-Generator Engineering Division, and Paul H. Knowlton, Assistant Designing Engineer. Princi-

Technology.[41] At first it was decided that the three contractors were to work on different phases of the project: at Princeton the attempt was to be made to develop a light attachment that could replace the standard brake as a field modification and which might possibly give immediate partial relief from obscuration; at Schenectady it was proposed to construct a high-pressure steam "gun" emptying into an evacuated chamber by means of which an attempt would be made to correlate the properties of high-pressure-ratio transient jets to proper sequences of steady state jets; and at Pasadena the basic work for a long-range program was projected. It was not always found feasible, however, to adhere to this program. Soon after the signing of these contracts Division 2 undertook to study the properties of muzzle brakes at the request of the Ordnance Department, and the contract for this work was given to the Franklin Institute.[42] As part of the general blast investigations being conducted under Division 2, the Princeton Station also accepted a project for the study of the characteristics of high-pressure jets by the interferometric method.[43] This work was undertaken at the request of the Bureau of Ordnance.

At first it was believed (on the basis of information from Ordnance) that the investigations should be limited to nozzles of small size that did not alter the recoil characteristics of the gun and did not increase the blast pressures at the rear; and it was emphasized that no asymmetry in the blast could be tolerated. All these restrictions were eventually removed, principally on the insistence of the Tank Destroyer Board.

The first experimental work was done at Princeton with a caliber .30 rifle and with the aid of spark photography. While the steam gun was being built at Schenectady, a caliber .50 medium-pressure air gun was constructed on which a great variety of nozzles and deflectors were tested. On the basis of the results obtained with these guns it was concluded that a satisfactory solution of the problem could be quickly achieved, and some of the more promising models were scaled up to 37-mm. size, this being

pal technical contribution was made by Sanford Neal, S. W. Evans directed the setting-up and operation of the steam gun, J. W. Eberle assisted Mr. Neal in setting up and operating the air gun, S. L. Bellinger worked with Mr. Neal and the Princeton Station in the development of the vital high-speed photographic technique, and Hans Kraft acted as consultant throughout in theoretical matters.

[41]Under the general direction of Dr. Clark B. Millikan, Professor of Aeronautics. Dr. E. E. Sechler was the project co-ordinator, directing the work of Rolf D. Buhler, who conducted most of the experimental tests, M. E. Bessey, who designed the spark discharge unit and time delay circuit, F. L. Poole, who designed the photocell equipment for field firing experiments and conducted calculations and experiments, and Mrs. R. H. Mulcahey, who did all the drawings, worked on the development of spark photography and gave general assistance in the experimental work.

[42]Under the general direction of Dr. H. B. Allen. Other personnel included Dr. N. M. Smith, project director, Dr. R. Eksergian, consultant, Dr. W. F. G. Swann, consultant, F. R. Simpson, mechanical engineer, G. N. Saccas, design draftsman, L. Streletz, machine shop foreman, G. Hill, machinist, and P. Casselman, technician.

[43]Under the Direction of Dr. R. Ladenberg.

the largest caliber that could be fired at Princeton; but when tests were made on this gun a serious complication appeared. The wind, which carried the dust raised by the deflected jet back into the line of fire, often produced long and serious secondary periods of obscuration. The effects of the wind had, of course, been anticipated, and fans had been used with the caliber .30 rifle to blow the dust raised at the sides of the gun into the target area, but the effects of this artificial wind were quite unlike those observed in the field.

A controversy arose regarding the mechanism of dust raising, and Slade demonstrated theoretically that the pressure gradients in the air produced negligible effects on the motion of the dust particles independent of particle size and that the selection according to particle size was due entirely to the frictional drag. High-speed motion pictures taken at Schenectady showed a secondary explosive rising of dust near the muzzle when the gun was fired close to the dust table. Kraft first called attention to this phenomenon, which was eventually attributed to the effect of the terminal rarefaction following the emptying of the gun. The recognition of this rarefaction effect turned out to be of great importance in understanding the performance of deflectors.

Since the smallest particle sizes, which are exceedingly sensitive to the motion of the air, always occur in natural particle-size distributions, it became apparent that the scaling problem presented serious difficulties and it was decided to experiment directly with the larger calibers. Systematic investigations with the caliber .30 rifle were continued at Pasadena. The importance of the small-scale work was recognized at Princeton but the scarcity of personnel there required that this program be greatly curtailed, though sufficient work was done to lead to important conclusions pertaining to general problems of design. A 76-mm. gun was procured and a range at Fort Dix was placed at the disposal of the Princeton Station. The shop facilities of the Palmer Physical Laboratory were then devoted largely to the production of full-scale models.[44] The range at Fort Dix proved inadequate for these tests as the sandy soil there was generally saturated with moisture. Only proof firings were made there. On the invitation of the Tank Destroyer Board, Camp Hood in Texas was selected for the field experiments.

The evaluation of the relative merits of deflectors turned out to be a difficult problem. At Pasadena, Buhler had developed an apparatus for recording the degree of obscuration which consisted essentially of a photocell absorption meter coupled with a recording galvanometer, and at Princeton,

[44]The Princeton University shop did a remarkable job in cutting these large-scale deflectors out of solid blocks of steel. Efforts were made to have some of the more laborious work done at other places, but at that time all machine shops were working at full capacity and could undertake no new work.

Fletcher used the idea to construct a recording device for use in the field. There remained the statistical problem introduced by the variability of the weather. This problem was never satisfactorily solved, principally because the traverse of the large guns was restricted by the narrow limits of the impact area, and during the six months when the tests were conducted the wind maintained a fairly constant direction with respect to the axis of the gun.

A deflector was finally recommended for standardization. This was a light unit with four conical baffles and large ports that gave the blast a slight upward deflection which produced no noticeable effects either on the elevating mechanism of the gun or on the flight of the projectile. The work of Buhler on the caliber .30 rifle had demonstrated the superiority of the conical baffle. The small-scale work at Schenectady and Princeton had shown that it required a deflector of large capacity to suppress the effect of the terminal rarefaction. The light unit that was recommended for adoption did not perform so well as other larger deflectors when the gun was fired at extremely low bore heights, but since a gun does not fire at those heights except from a prepared position it was decided that the deflector could then be used in conjunction with a blast mat.

During the full-scale tests it was observed that multiple-baffle deflectors tended to suppress flash though one or two baffle deflectors generally enhance it. The flash from the 37-mm. gun was effectively suppressed with a four-baffle deflector, that of the 76-mm. gun was eliminated with an eight-baffle deflector. The latter deflector, however, produced no visible effect on the flash of the 90-mm. gun.

The solution offered by the unit recommended for adoption was only partial. Three possibilities for controlling the blast more definitely were considered. The blast could be deflected completely upward, a valve could be used to close the hole behind the projectile and lengthen the emptying time, or a nozzle could be developed that turned the gas efficiently through 180 degrees so that it could be piped to the rear of the gun. These methods required radical changes in the design of gun and mount and a fuller knowledge of the emptying characteristics of a gun than were available at the time. For calculations of the thrust due to unsymmetrical deflection of the blast, Buhler developed the work that had been initiated by the British; and Fletcher and Slade initiated a complete investigation of the interior ballistics of a gun after shot ejection.

A valve was developed by Cejka at Princeton that worked surprisingly well on the caliber .30 rifle, and Slade developed a nozzle, built only in small scale, that intercepted a large fraction of the blast and compressed and turned the deflection jet so that it could be piped to the rear. This nozzle of course developed very high braking efficiencies.

At the Franklin Institute, Simpson investigated systematically the effect on recoil energy absorption of changes in the proportions of a nozzle-baffle configuration. Very high braking efficiencies were obtained with some of these designs.

All of this work stopped by the termination of contracts following V-J Day and the consequent elimination of the emergency. However, enough had been done to offer real possibilities for improved future guns, which will always be better if they can fire faster.

## Getting the Messages to Garcia

By the time the division was aggressively in the war, the risk of attack on our own cities was slender, although the public and the OCD did not always concede this. The division had at its disposal, however, the most extensive amount of data in the United States on the effect of German bombs on all sorts of British installations. Evidently this and similar knowledge ought to be brought to bear on the attack. The opportunity arose when Burchard was invited to become a member of General Arnold's Bombardment Advisory Committee under the chairmanship of Colonel, later Brig. General B. F. Sorenson. At the very first conference the targets under consideration included Siemensstadt near Berlin, the Zeiss and Schott works at Jena and similar important elements of the German economy. It was evident at once that the Air Force was overoptimistic as to the damage small bombs were likely to cause. To prove this point, or rather to demonstrate it, since the evidence available was scarcely proof, Burchard brought to a subsequent meeting some of the very careful damage reports made by Regional Technical Intelligence Officers of the British Ministry of Home Security. These reports illustrated, with photographs and measured drawings, the damage which had been done to specific buildings of several types when struck by German bombs of various sizes and weights. For the discussion of the moment, they served their purpose; they also served to convince the division that a more formal and complete presentation of the data in its files should be available to operators everywhere, and especially to those who were entrusted with the selection of weapons for individual attacks. This meant a sort of field manual. It meant that the existing field manual TC-50, the official doctrine for bomb selection, was badly out of date. Despairing of immediate revision of that manual, the division set forth on what became one of its major projects, the loose-leaf book of data on the "Effects of Impact and Explosion" which ultimately served many other customers as well. Later, the division was called upon to advise in the revision of TC-50 and also in the preparation of a corresponding manual for the Navy, and at the end all these data compilations were in essential agreement. None the less, it can reasonably be claimed that the division

pioneered in the presentation of attractive, clear, easily comprehended data of many sorts for the use of combat commands.[45]

It was recognized at the outset that this form of service is quite different in nature from the research which produces the information used. A research scientist is always keenly aware of the gaps in his knowledge, and usually emphasizes those features of a current theory which are least satisfactory — because it is just these features which are most significant for further research. The whole training of a research worker is toward withholding publication until a satisfactory answer is obtained. On the other hand, in order to be as useful as possible in wartime, a collection of data sheets should be as complete as is possible at the time, not waiting for a final definitive answer to each problem, but rather presenting the best-considered hypothesis on the subject, and relying on successive amendments to extend its scope and correct any errors shown up by subsequent research. Thus the data sheet manual would come as close as printed matter can to providing both a convenient tabulation of data already well known and also the best-considered hypotheses on problems still under study. Accordingly a distinct group was organized to carry out the functions of:

1. collecting pertinent information already existing in various reports,
2. on subjects for which research programs were under way, assisting the research workers in the explicit formulation of the best current knowledge,
3. on subjects for which no research programs existed, carrying out analyses on whatever observations existed, and
4. preparing the results in clear, compact, and convenient form.

The data sheets were of several general types. All aimed primarily at supplying information about the behavior of weapons upon reaching their targets, such as, for example, the penetration of various materials or the effect of an explosion in producing target damage through blast or earth shock. In order to apply this information, however, it was necessary to know more than was readily available at the time about the delivery of the weapon to the target. Problems of accuracy were not taken up, but it was found desirable to include as one general type data on such things as projectile

[45] The work was initially organized by Burchard under the direction of Slutz, and carried out by members of the Princeton University Station of Division 2, Bleakney being the head of the Station. When Slutz went to England in the fall of 1943, Ira M. Freeman took charge of the group working under Bleakney. From September 1944, until the end of the war, this group was under the direction of Stipe, Jr. Taub assisted Bleakney in the direction of the policy of the group. Practically every member of the Princeton University Station worked with the group at one time or another; each project contributed in its own field. The persons who would be considered as members of the Data Sheet Group itself rather than of one of the other projects of the Station were Robert H. Dietz, Frederick L. Klauck, Nathan M. Newmark, Ieoh Ming Pei, E. J. Tan, Joseph S. Wise and A. Addis Ziegler. The Incident Summaries were prepared by Leroy A. Brothers. Frederick G. Roth was in charge of the layout and reproduction of sheets from the formation of the group until its dissolution. He was assisted during 1944 and 1945 by William E. Bergman.

velocity as a function of range, and the striking velocity and striking angle of bombs as a function of the altitude and speed with which they are dropped. These latter data are obtainable from the complete tables, but were not included in the condensed tables furnished to bombardiers. The Ballistic Research Laboratory, of Aberdeen Proving Ground, prepared for the division tables giving this information in a form such that it could be calculated for any bomb from the ballistic coefficients. This was prepared by the Data Sheet Group in direct graphical form for each of the bombs in use by the American Services, and these graphs made up one of the first sets of sheets to be issued. Additional sheets were added to this series as additional bombs came into use, and later it was found possible to make a particularly simple graphical chart which will enable the determination of any bomb's striking velocity and striking angle when dropped from low levels even if the plane is diving or climbing when the bomb is released.

When the issuing of data sheets was started, there was available only a small amount of information where the effect of a bomb on a specific type of structure had been put together and analyzed sufficiently to permit a quantitative estimate of the probable effect; the data consisted rather of a collection of observations on the damage produced when bombs had fallen on England during the early part of the war, without any careful summarization. Even these reports of incidents were not generally available here; the division had a collection of them unique in this country because on his first mission to England Burchard had recognized their significance and arranged with the Ministry of Home Security to have a carefully selected group of these reports — reports not normally distributed outside that Ministry — reproduced and sent to the division. The structural damage produced by a bomb will vary widely even in situations which would appear to be equivalent, so incidents chosen at random might give a very false impression of the behavior. The incidents in this group were selected by engineers familiar with large numbers of such incidents as being typical of the effect to be expected for the type of bomb and structure involved, and incidents were included which covered the range of bomb and structure type which would generally be met; their usefulness in describing the effects of bombing has been mentioned previously in connection with the Bombardment Advisory Committee. Accordingly an important part of the early program of the Data Sheet Group was the summarization of these incidents into single-page presentations which would include all of the significant features and make them readily assimilable through clear presentation. These were the Incident Summaries. They proved very valuable not only as an immediate source of information not otherwise summarized, but continually by giving a semiquantitative picture to persons without direct experience of bomb damage, and also by indicating the interconnection between the various types of damage.

For the two types of sheets just discussed, the division's function was largely one of locating information already existing, then digesting and disseminating it; it is only humanly natural that a more parental feeling existed for those problems whose solution grew from a more extensive analysis within the Division. In some cases this resulted from the other programs of the Division, and in some cases — such as the analysis of the vulnerability of tactical targets — directly through the work of the Data Sheet Group. Each of the other projects resulted in information suitable for inclusion in the manual, and the manual proved to be an excellent means of making this information promptly and conveniently available to the field. Sheets in this group included those on penetration, perforation and scabbing of concrete, those on the effect of underground explosions on concrete walls, and those on blast pressures and impulses in air.

It has already been mentioned how the incident reports were obtained. Later the British Ministry of Home Security was kind enough to supply the division with special copies of their unpublished files compiling data on damage to strategic targets such as industrial buildings and equipment. These compilations were used in preparing sheets dealing with such damage, and indeed were almost the only source of such information until Allied attacks commenced and aerial photographs became available. In the fall of 1943 the need was felt for additional data on tactical targets such as railroads and bridges, and Slutz undertook a trip to England for such data. Later, when it was once more possible to go to the European continent, White, Taub, Dietz and Rawdon-Smith made ground inspections of targets that had been the subject of Allied attacks, prepared reports on the damage sustained, and made arrangements for the division to receive the reports prepared by other damage surveys.

One of the most interesting contributions of the Data Sheet Group was a series of studies on the vulnerability of specific types of targets, such as airfields, oil storage tanks, bridges, tunnels, etc. These investigations, undertaken at the request of the Joint Target Group, utilized all available information and methods of approach. Thus results of model experiments carried out at Princeton, fundamental data from the division's basic programs, probability studies from the Applied Mathematics Panel, and considerable operational data from the Air Force Operations Analysts were all pooled, evaluated and reduced to concrete recommendations and to Data Sheets.

Some of the most important users of the data sheets were the Operational Analysis groups attached to the various Air Forces — the very groups to which the division's training program supplied personnel. The Data Sheet project worked very closely with the training program from its inception. While in training the personnel became familiar with the Data Sheet manual, which was one of their textbooks. In any interval which occurred

between the end of their formal training program and their assignment to the field, they customarily worked with the Data Sheet Group, thus both furthering the work of the group and adding to their knowledge of weapon effects. Later, when in the field, they were in the position of users of the Data Sheet manual, and frequently sent back to the Division further data, suggestions for additional subjects to be covered, or corrections. In this way the group working on the Data Sheets was able not only to receive important data, but also to keep in touch with persons who were using the manual frequently and so knew its needs in a very direct way.

One example of a Data Sheet developed in response to a direct request from an Operational Analysis Group is the one for calculating the shadow ratio used in aerial photographs to determine the height of buildings. While this subject would not normally have been included in the Data Sheet manual, word was received that the calculation of this ratio was proving laborious and time-consuming when large numbers of photographic sorties were studied. By the use of a special nomogram, it was found possible to reduce this problem to seven straightforward steps.

The solution of the above problem is indicative of the approach that was taken to each sheet. It was not considered enough to obtain the information and compress it onto one or a few pages — that could have been done merely by photographic reduction. Instead every effort was made to present the results in a manner which would enable them to be used with a minimum of effort and a minimum chance for errors of understanding. To this end considerable thought was undertaken on the various ways of representing formulas — algebraic, graphical, nomographic, and tabular. Oftentimes a nomogram could be worked out which would give a simple representation of an awkward function of several variables; at other times a graph was used on which some experimental points were plotted to warn the user about the extreme scatter of the data; at still other times a table was believed to be the most convenient presentation.

Initially the project was solely one of Division 2, and the Data Sheet notebook was then titled "Effects of Impact and Explosion." It was from the first realized that for completeness data from Division 3 on rockets and from Division 11 on incendiaries should also be in the notebook; it was later arranged to include these data, making the project one jointly of the three Divisions. The name accordingly was changed to "Weapon Data — Fire, Impact, Explosion."

Several sheets dealing with incendiaries were included, but the conclusion of the war cut short this extension of the book's scope.

The initial distribution of the notebook consisted of only 50 copies, and was made on a high echelon. The demand for it was so great, however, that in spite of its security classification the distribution grew by leaps and bounds. Eventually more than 350 copies of the large-size notebook were

distributed and nearly 600 copies of a pocket-size edition. In addition to this direct distribution the Eighth Air Force reproduced about 600 copies for their own use, and later USSTAF requested 1500 copies of any supplementary sheets in order to continue this distribution, but the war ended in Europe before the next set of supplementary sheets was distributed.

The Data Sheets were only one answer to the very difficult general question of how to get the results of research into the right hands quickly. The division issued the usual formal OSRD reports, each containing a more or less complete treatment of some problem. These reports are necessary but it is almost impossible to get them written and issued as promptly as is necessary for the tempo of war. Therefore, in the summer of 1944 several series of monthly reports were begun, each of which contained short reports on a group of related topics. Whenever a program reached a point which justified reporting, considerable pressure was brought to bear on the contractor by the division to get a contribution for one of the monthly series. These were prepared carefully but promptly and were not intended to be either final or complete. They undoubtedly helped greatly in getting the results of the division's work into the hands of those who could use it with the minimum delay. Very few of these preliminary reports were found to require later correction.

## Men for the Bomber Commands

The early foray on the division for personnel to serve as advisers to the Air Forces in the theaters, initiated by Major Leach and resulting in the first instance in the temporary loss of Professor Robertson, made it clear that some insurance needed to be provided if the division were not to be wrecked through loss of its key personnel. The Air Force needed these operations analysts badly. They could be trained from a group of men, civil engineers and architects largely, who were not in such short supply as the physicists at Princeton. Princeton was well fitted to train them. Accordingly an agreement was entered into in late 1942 with the Air Force whereby Princeton would train and deliver cadres of such personnel ready to serve the various bomber commands as damage analysts.

The first group of six men, all with previous training and experience in architecture or engineering, were brought to Princeton in the spring of 1943 for formal training, and were assigned to the Air Forces on completion of the course.[46]

[46] The original six were: (see also footnote 49)

| | | |
|---|---|---|
| Bissell Alderman | — | VIIIth Bomber Command |
| Albert W. Arneson | — | FEAF (SW Pacific) |
| George Hartman | — | China — Burma — India |
| George W. Housner | — | XVth Air Force |
| Charles U. Kring | — | VIIIth Bomber Command |
| Derald M. West | — | IXth Air Force. |

Of these pioneers Arneson was killed in action at the landing on Borneo.

The work of this group of men was so well received that there were immediate requests for additional men of similar training and abilities. Several other groups were formed and trained, and by the end of the war forty men had been through the regular training course and several others had taken part-time training. Eight of the men were naval officers. All of the others were civilians and retained their civilian status when attached to the Air Forces or other organizations as consultants.

The last group of four men completed their training in August 1945. One man had his baggage on a plane in California, ready to go to the Pacific, when word of the Japanese surrender came through. Another was in Washington, having assimilated various vaccines and packed his baggage, receiving final instructions preparatory to leaving the country. Two others were awaiting assignments that never came through.

Most of the men trained by the division were assigned to Operations Analysis Sections of the various Army Air Forces. These sections included specialists in many fields, and the Division 2 trainees were the specialists in weapon selection and damage assessment. Their individual duties varied, but all were concerned with several phases of operations planning, and some were influential in decisions taken at higher levels.

These men usually made the selection of bomb and fuze to be used against each target, and determined the size of the force required to attain the objective of the raid. After the attack they studied aerial photographs to assess the damage and to gather new information to be used in planning future attacks. To do this work they had to be kept informed of all new developments in weapons and the application of weapons, and needed close liaison with similar groups in other commands so that they could know and use the latest tricks of the trade while these were still fresh. The personal liaison provided by the common background of the training was an unofficial but important factor in keeping the men advised of new developments; all reports issued by one group were regularly distributed to the others, and reports were sent back to the Division as aids in training new men. The loose-leaf notebook *Weapon Data—Fire, Impact, Explosion* (formerly *Effects of Impact and Explosion*) was of course distributed to them through Major (later Colonel) Leach as a condensed set of reference material. Their continuing interest, advice, and requests for new sheets for this notebook were very important in guiding its development.

Men to be trained as Operations Analysts for weapon selection must have a good knowledge of engineering structures, mathematics, and applied physics. They must also have better than average resourcefulness and ingenuity and yet be able to work well as part of a team. Such men are hard to find in the midst of a manpower shortage, and it is difficult to keep them out of the clutches of man-hungry draft boards, but they were found, kept, and trained.

The training course at Princeton[47] lasted for six or eight weeks, plus as much time as possible in visiting other organizations and in examining typical industrial plants. All of the men had a review course in mechanics and a special course in mathematical probability and its applications to bombing problems. They were given reference material on weapon effects and instructions in the use of this material and the finding of additional material. Lectures were given by members of the research staff of the Division on the effects of air blast and underground explosions, terminal ballistics of armor and concrete, and the effects of weapons on targets. Training films, loaned by the Army Air Forces, were used for instructional purposes. The groups received additional training by other organizations listed here. The U.S. Navy Bomb Disposal School gave the men a short course on the characteristics and properties of bombs and fuzes.[48] NDRC Division 11 sent representatives to Princeton to give lectures on the properties and effects of incendiary bombs. Several of the groups were given additional training in the theory of probability as applied to bombing and in methods of analysis of data by the Applied Mathematics Panel. Many, but not all of the men received further training at the Army Air Forces School of Applied Tactics (AAFSAT). The last group of trainees made a trip to Pasadena to receive instruction on rockets and their effects from Division 3.

A feature of the later training programs was visits to various industrial plants, under the guidance of J. A. Wise, a structural engineer on the research staff. The plants were examined for vulnerability to bombing, and the critical nature of certain equipment and operations was discussed with the managers of the plants, from the point of view of vulnerability of this equipment to bombing. In some instances plant managers or their assistants became excited, believing that an attack on the factory was imminent and that our men had come for the first of "before and after" examinations to assess bomb damage. Immediate explanations eased their fears.

The importance and usefulness of the work of these Operations Analysts is entirely out of proportion to their number. Less than forty men, spread over the world in groups of one or two and working with the several Army Air Forces, Naval Task Forces, and planning groups in Washington, were very influential in planning the bombing attacks that made our air power one of the most important factors in World War II. Many men have made

[47]The men were trained by H. Scheffé and J. L. Brenner (mechanics and probability); Wise (mechanics and structure analysis); Freeman (mechanics, general supervision of studies); Beth (concrete); Curtis (armor); Lampson (earth shock and air blast); Stipe, Jr. (target analysis and soil); Taub (target analysis and air blast); Slutz (reference material, supervision of studies); the course was under the general supervision of Burchard (at first) and Beckwith (later).

[48]The officers in charge of this school were at all times exceptionally co-operative with the division, not only in training personnel, but also in supplying full ordnance information for the Division work and for the Data Sheet program.

more important decisions at higher levels, but few groups so small have had such a great effect on the techniques and philosophies of aerial warfare. Warfare is a branch of engineering, and aerial warfare is a very specialized branch. Men with the proper technical background and training must plan the details needed for good engineering and for the efficient destruction of enemy installations.[49]

[49]Listed below are the full names of men in the various training groups together with the approximate completion date of their training, their sponsor and their destination where known:

| | | |
|---|---|---|
| Alderman, Bissell | Completed training Spring 1943 | 8th AF |
| Kring, Charles U. | " " | 8th AF |
| Housner, George W. | " " | 9th–15th AF |
| Arneson, Albert W. | " " | 13th AF |
| Hartman, George | " " | 8th–15th AF |
| West, Derald M. | " " | 9th AF |
| Reinke, Leonard H. | Completed training Spring 1944 | 9th AF |
| Packer, George | " " | Joint Target Group |
| Olson, Carl O. | " " | 14th AF–CBI |
| Mayer, David | " " | CBI |
| Little, Robert A. | " " | A2 |
| Thorson, Oswald H. | " " | 9th AF, Navy, JTG |
| Lindstrom, Lester J. | " " | 7th AF |
| Nettleton, Douglas A. | " " | New Dev. Div., Washington, P.R. |
| Coker, Joseph D. | " " | OPNAV16 (Navy) |
| Grassy, Richard | " " | OPNAV16 (Navy) |
| LeGalley, Donald P. | Completed training Summer 1944 | Central School for Flexible Gunnery, Texas, AAFTC |
| Barron, Maurice | " " | 20th Bomb. 8th AF |
| Burson, Richard D. | " " | 20th AF, 21st Bomb. Comd. |
| Rose, Ellsworth L. | " " | Washington, D.C. |
| Picardi, A. L. | " " | Oper. Research Group, Washington |
| Proctor, J. Virgil | " " | 21st Bomb. Comd. P.R. |
| Reade, Maxwell | " " | Joint Target Group |
| Claessens, Frank A. | " " | 11th AF, JTG |
| Otto, Arthur L. | " " | P.R. |
| Dietz, Albert G. H. | " " | Oper. Research, Hawaii |
| Byfield, Abbott | " " | P.O.A. Hawaii |
| Bowen, Earl K. | " " | P.O.A. Hawaii |
| Carter, H. G. Lt. (jg) | Completed training Fall 1944 | (Navy) |
| Koper, A. Lt. | " " | (Navy) |
| Selig, D. Lt. | " " | (Navy) |
| Speake, P. M. Lt. (jg) | " " | (Navy) |
| Weichelt, J. A. Lt. | " " | (Navy) |
| Husted, E. S. Lt. Comdr. | " " | (Navy) |
| Gray, Nomer | Completed training Summer 1945 (June) | |
| Porteus, John H. | " " " | |
| Shotwell, Henry T. | " " " | |
| Sollenberger, Norman J. | " " " | |
| Hartman, George | " " " | (retraining) |

## To Burn or to Blast?

The purpose of bombing is damage. The effort that underlies a bombing attack is gigantic: building airplane and bomb factories, development and production of the myriad accessories, training mechanics and crews, transporting shiploads of equipment and supplies — an effort that absorbed millions of American men and women. But the intended purpose of this stupendous effort can be summed up in the one ugly word: damage.

To do damage effectively, a bomb suited to the structure of the target must hit the target. In the last analysis, it all comes down to two problems: (a) accurate and safe delivery of bombs and (b) correct choice of bombs or combinations of bombs and the correct timing of their fuzes. The problem of delivery — and the intricate instruments that it involves — was receiving keen attention in the Air Forces and in the NDRC, and the manpower allotted to it was commensurate with its importance. But the sister problem of bomb choice was in a confusing state, and the personnel assigned to handle it, although trying its best, was inadequate in numbers. On the higher levels it was apparently believed that it was known what bombs can do; but this was far from the fact.

The British were naturally ahead of us in the knowledge of bomb damage, because the evidence of what bombs can do, or cannot do, was spread in their own cities, for all to see. The British "incidents," as examples of damage were called, together with theoretical studies and the results of model-scale tests, thus formed the basis on which the probable effectiveness of various practicable bomb-loads was judged. But as the bombing of Germany and occupied countries progressed, another source of evidence gradually became available: the postattack photo-cover, that is, the postattack photographs of a target taken by Allied reconnaissance planes, to be compared with the preattack photo-cover, which revealed the structure of the undamaged target in reasonable detail. It thus became possible to estimate bomb effectiveness from much more tangible data, and to correct misconceptions in the earlier ideas on bomb choice; in other words, more direct weapon analysis became possible.

The opportunity was there, but the necessary manpower was not. At the Operational Analysis Sections of the Air Forces, and in civilian organizations, such as the British establishment at Princes Risborough, attempts at improved weapon analysis were made; but the day-by-day task of recommending bombs for the attacks to come left but little time to look back at the results of previous attacks and to reformulate the ideas in the light of experience. At places like the Princeton Station of Division 2 and the headquarters of Division 11, where interest in weapon analysis was obviously great, the work could be done more deliberately; but the difficulty was that of obtaining accurate data — as the subsequent work showed, to get reason-

ably reliable information on bomb-loads and on the sequence of events in an attack, it is imperative to dig right into such documents as the narratives of bombardiers.

Project AN-23 was organized in the summer of 1944 as a consequence of the efforts of Rojansky,[50] when it became quite clear that work in weapon analysis was badly lagging, and that therefore bombs had sometimes to be selected without the benefit of critical study of experience; concurrently, the newly organized Joint Target Group of the Joint Chiefs of Staff, then under the leadership of Commander Francis Bitter, U.S.N.R., was taking steps to put weapon analysis in Washington on a more adequate basis. Three things were recognized by the planners of AN-23 from the beginning: To make the task manageable, it should be restricted; and industrial targets were chosen as most needing immediate attention. The project should be a joint enterprise of Divisions 2 and 11; the controversy on the relative merits of high-explosive and incendiary bombs (as well, incidentally, as the controversy on small-versus-large high-explosive bombs) on industrial targets was raging at the time, and the NDRC group should be competent to look impartially into both sides of the question. And the first step should be to get as reliable data as possible, directly from the theaters of operation.

The over-all supervision of the project was undertaken by Hoyt Hottel of Division 11, who set out to persuade the Air Force that the men assigned to the project should have access to firsthand data, on past attacks, at Operational Analysis Sections in the European Theater. Progress was at first slow, because of the prevalent impression that all that one could possibly need was available right in Washington. Fortunately certain officers — Lieutenant C. J. Hitch, USAAF, was one — were fully aware of the actualities. By September, Hottel succeeded in getting a promise of the necessary Air Force credentials, and shortly afterwards the seven men assigned to the project were on their way to the village of Princes Risborough, where Sir Reginald Stradling, Chief Adviser to the Research and Experiments Department of the Ministry of Home Security, had arranged for working facilities and living quarters.

The group consisted of Bohnenblust (Mathematician, Division 2), E. B. Gerry (fire engineer, Division 11), Hansen (structural engineer, Division 2), G. W. King (chemist, Division 11), J. Neyman (statistician, Applied Mathematics Panel), Rojansky (physicist, Division 2), and H. L. Webb

[50]Rojansky and Hansen had been sent to England in the fall of 1943 by Princeton Station as its visiting representatives. On his return in May 1944, Rojansky worked hard at interesting others in the problems, until finally a Joint Army-Navy Project AN-23 was established with a team of personnel provided by Division 2, Division 11 (incendiaries) and the Applied Mathematics Panel, under the general direction of a committee whose chairman was Professor Hoyt Hottel of Massachusetts Institute of Technology, and whose members included E. P. Stevenson, Chief of Division 11, Wilson, Chief of Division 2; R. Ewell, Division 11; Rojansky, Division 2; Bleakney, Division 2, and as Secretary, Kelly, Division 2.

(librarian, Division 2); administrative responsibilities were undertaken by Rojansky. At Princes Risborough the seven men were greeted with open arms; for Professor W. N. Thomas, in charge of the establishment there, Mr. R. Leader-Williams, Dr. R. B. Fisher and Dr. J. Bronowski, to mention but a few of the British colleagues of AN-23, were well aware of the need for the work mapped out for the project.[51]

The group stayed in England about eleven weeks. The first two were spent in a detailed study of the Princes Risborough methods of analysis. The next two were spent similarly at the Operational Analysis Section of the Eighth Air Force (headed by Lieutenant Colonel Leslie H. Arps, from whom the group received every needed facility), and at the Operational Research Section of the British Bomber Command (where Mr. B. Ross proved to be a most valuable consultant). Then the work of data collection began, with some of the seven men in one place and others in another, as the needs of the day required. The data consisted of attack information, pre- and poststrike photo-interpretations, damage analyses, function analyses, and the like. Concurrently, plans for the analysis of the data were being laid, to be discussed with the British experts and to be carried out after the return of the group to the United States. As the work progressed, it became clear that certain key data could be obtained only from the files of the Fifteenth Air Force; and so, after nine weeks in England, Bohnenblust and King went to Italy, where they continued to collect data with the co-operation of Dr. George W. Housner, who headed the Operational Analysis Section.[52] The group reconvened in the United States at the end of December.

After the tedious negotiations in Washington and the long uncertainty whether the unglamorous mission for damage data would get Air Force credentials, it was refreshing for the group to work within the sound of bombing attacks with men who knew the difficulties of the problem at first hand and who, except for a few, were anxious to help in putting bomb selection on a sounder basis.[53]

Perhaps the most intricate co-operative enterprise was the collection of data on the R.A.F. attack on Kassel (October 22, 1943), supervised by Gerry; it involved several American and British organizations and took about 700 man-hours. It requires much effort to unravel the structure of a

[51]At the London OSRD Mission, the group was assisted administratively by Messrs. Bennett Archambault and J. A. Britton, while Dr. Geoffrey Broughton, representing Division 11, advised the group on technical matters.

[52]One of the original six trained at Princeton. See p. 292.

[53]The work was co-operative throughout; and the names of Messrs. D. Beecher and S. W. Sternfeldt (Ministry of Home Security), Lt. Col. P. C. Scott and Dr. J. A. Clarkson (OAS, Eighth Air Force), and Major F. Sanborn, and F. Lowenstein (Americans attached to Princes Risborough) should be mentioned especially. For about a month, the Chemical Warfare Service assigned Captain B. Rust and Sergeants G. Pavik, Jr., and E. P. Lucas to work with AN-23.

target, to make sure what the bomb-loads were, to establish the sequence of events in an attack, and to assess the physical damage done, even without yet ascribing it to particular bombs!

After the return of the group to the United States, Neyman went to his statistical laboratory at the University of California, to undertake with his staff there a general mathematical analysis which would combine into a single whole not only the bombs and the damage that they cause, but also the accuracy and the bomb-release characteristics of the aircraft. Bohnenblust, Gerry, Hansen, King, Rojansky, and Webb set up shop at the Pentagon, where for a time[54] they were an integral part of the Physical Vulnerability Section, Joint Target Group, then headed by Lt. Comdr. W. W. Timmis, U.S.N.R., with Lt. C. J. Hitch, USAAF, as Deputy. The attention of AN-23 was concentrated in Washington on extracting from the available data the information, incomplete though it might be, which could be put into immediate operational use. Eventually the analysis was extended to include data that began to come in from the Pacific.

As the work of AN-23 was nearing its goal in the summer of 1945, Rojansky and later Bohnenblust had to leave because of long-standing commitments elsewhere. The responsibility for the project was taken on by King, who carried the work to completion.[55]

The data brought by AN-23 from Europe cover 36 American attacks on industrial plants in Europe. It may appall the reader to learn that of the hundreds of such attacks made by our Air Force and studied by AN-23, only 36 were found to be sufficiently well documented, damage-assessed, and not too confused by too great a variety of bombs, to permit reliable weapons analysis. But such are the facts!

The data of these attacks on European targets were reduced to various parameters to be used in a general theory of bombing. Values of the mean area of effectiveness, the probability of starting a fire, and the extent of spread of fire were found for the 500-pound U.S. GP bomb against various classifications of structures, and various types of contents. The probability of starting and the extent of spread of fire were found for the M47 and the M50 Incendiary Bombs against various classes of buildings and various categories of building content.

In addition the group was called upon to give advice to the Joint Target Group on the theories of bombing to be used in planning the attacks on Japan. The group was called upon to help draft a program of "Controlled Attacks" to be carried out on Japan by the 20th Air Force. In this program, the effects of various bombs, such as the 4000-pound LC, the M69 incendiary alone, and the M69 incendiary with high-explosive bombs, were to be determined, by carefully controlling the variables of the attack. Several of

[54]From May 15 to July 1, 1945.
[55]Under contract with Arthur D. Little, Inc.

these huge-scale experiments were carried out, and the data were analyzed by the AN-23 group.

The question of the best combination of incendiary and high-explosive bombs for a particular industrial installation can now be answered more reliably than it could be answered before, and furthermore methods have been developed which could be applied to future weapon selection problems.

## In Retrospect

These then were the principal projects of the Division and these were the ways they were carried out. In retrospect the workers could look with more or less satisfaction on certain tangible accomplishments, an authoritative basis for designing and selecting plastic armor, a reference library of distinction in its field — the Mecca of nearly every person who was interested in the general work — the definitive information as to whether a given weapon could be expected to perforate a given installation, data sheets resting thumb-worn on the desks of the operational commands of our Air Forces throughout the world, and men, small in number but strong in influence, working on the problems of the bomber commands from the forces of the 17's and the 24's in Europe to the forces of the behemoths of the Pacific.

Less spectacular but of more enduring value to the defense of the nation, the Division had materially extended the frontiers of knowledge as to the basic principles of perforation and penetration, of behavior of metals under rapid rates of strain and excessively high pressures, of projectiles traveling at previously unevaluated speeds, and of targets in response to such fast missiles; it had opened the gates to new frontiers in the highly important subject of air burst, shock in air and earth, the Mach effect.

Of the utmost significance, in little for this war because of delays and resistances, in large for another, it had established important relations between incendiary and high-explosive bomber loads, the essential facts for the design of projectiles to be propelled at high velocity, and the desirability of an air burst of a large explosive instead of the ground burst of a small one. Against unimaginable opposition and only through the fortitude of two men who refused to be subdued, it had created a new training weapon for gunners in bombers which stood to save the lives of many an American bomber crew.[56]

The account has made little effort to establish in the minds of the reader the many technical difficulties, discouragements and reversals which occurred during the transition from no knowledge to a good deal, and which are passed over lightly here. These are the normal fare of scientific research and no scientist would find them surprising, though it might be well for

[56]The account of this will be found in Chapter XXX.

the public to realize that nothing is a miracle and almost everything is the result of hard work and straight thinking.

There were, however, other difficulties with which scientists had not been trained to deal. They came largely as a surprise, although perhaps no social scientist would have been surprised that they existed. They were by no means trivial and are not to be accepted as the necessary lot of those who are trying to improve technical proficiency. Because they were not trivial, because they were not necessary, to omit them from history would be a distortion of all truth and a delusion to the reader. Some consideration of them is therefore afforded in the following chapter.

# CHAPTER XXIX

## *SAND IN THE GEARS*

IN A NATION where scientists have traditionally known little about the weapons of war and where the departments of government devoted to war were permitted by parsimonious national policy to maintain only pitifully small peacetime war research establishments, mobilization for war research is not easy.

Of the general difficulties confronting any administrator plunged into governmental work, it is unnecessary to speak. Civil Service limitations, constant changes of policy with respect to details imposed by actions of other agencies, impossibilities of preparing significant budgets in a period when time was flowing out so rapidly and when the opinions of today had to be reversed tomorrow — all can be dismissed forthwith as part of the job of administration to be anticipated in any such endeavor. It is fair to say that those charged with divisional administration could better understand and appreciate the care with which the people of "P Street" [1] had paved their way than those farther away from administration, who were often irked by the torrent of mimeographed paper which flowed from that office. There were, of course, times when centralized dicta were vastly more upsetting to the divisional administrations than the central office could know. Two cases are worth rehearsing here for the general light they shed on such operations.

In the fall of 1944 everyone was very optimistic about the end of the war; other factors were doubtless also at work, which, unknown to the Divisional Chiefs, prompted the Director OSRD to look towards the early demobilization of his organization. The Office of the Chairman NDRC at that time issued a series of memoranda aimed in that direction. For Division 2 these memoranda and the resulting attitude of the administrative offices had the adverse effect of making it almost impossible for the Division Chief to secure the additional personnel needed to replace resignations; if he were to believe the memoranda, it would not be proper or possible to

[1] The more or less affectionate term for 1530 P Street, N.W., the peacetime home of the Carnegie Institution of Washington, containing during the war the offices of the Director, OSRD, the Chairman, NDRC, the Executive Secretary of both OSRD and NDRC; the services associated with the office of the Director, OSRD, such as the Legal Section; and, variously, the Office of Scientific Personnel, the Fiscal Section, the Travel Section, the Priorities and Property Control Section, the Security Section, and the like.

ask a man to accept such a short-time job; if demobilization were imminent, further funds for research would not be available. As things turned out, the organization did not demobilize for well over a year and a half, and the result of this was that the Division was never thereafter properly staffed. The circulars concerning demobilization had a severe effect, moreover, on the morale of the research men; they tended to make these men feel that any work they might continue could not affect the outcome of the war. Since they had embraced this work with little enthusiasm other than the enormous one engendered by patriotism, their instinct was to terminate the work as rapidly as possible. The point of this is not, of course, that the high command made a mistake in its estimates of the end of the war — this was a mistake shared by all the high commands and perhaps justifiable in the circumstances — but rather that it followed a policy with respect to the smaller units which on the one hand tended to discourage filling gaps in personnel and doing assigned work and on the other hand continued in the end to appropriate funds for further work. What this really adds up to, as an administrative sermon, is that central offices need to consider with care the total effect of their edicts on their own personnel.

Equally serious and extending over a longer time was the policy of P Street as to the qualifications and requirements for members. This policy was certainly not the choice of P Street but was imposed by Government rules and a long established public opinion, both of which the Director OSRD evidently struggled to modify. The public, and especially their legislative representatives, have a not entirely unjustified suspicion of experts from industry who, the public believes, will be unduly influenced by private interest in their determinations. This tends to manifest itself in witch-hunting. For example, there was at one time a great stir over the fact that an important committee of metallurgists was made up entirely of men from the great metallurgical industry (this was the sponge iron controversy). As a matter of fact, equally competent men could not have been found outside the ranks of industry. It was, moreover, the general experience of Division 2 that such industrial experts did, in fact, subordinate their commercial interest to patriotic truth throughout the war. Nonetheless, the suspicion was there and was never dissipated, and no administrator of a public organization could ignore it. More serious and also a reflection of the same temper were certain Federal Code provisions which made persons who remained attached to private enterprise while serving the Government subject to serious criminal prosecution for acts relating to their own employer. Since the ablest men could, on the whole, not afford, either financially or spiritually, to leave the atmosphere of private enterprise for the atmosphere of civil service, this posed a major dilemma which was never adequately solved, although the Director OSRD persisted in his efforts to solve it and did secure some alleviation. In its broader aspects this is a matter for discus-

sion in a general OSRD administrative history. Here we are concerned only with how it affected the selection of division members.

In the first days of NDRC these considerations were not rigorously applied and the selection of consultants was based purely on their technical qualifications. At the beginning of the reorganization, division members were similarly selected. In the work of Division 2 it was inevitable that all the most experienced men were either associated with explosives companies or were in the employ of large university contractors who had begun to specialize in ballistics and explosives. These men naturally became the Division members of Divisions 2 and 8. But there was a steady trend on the part of P Street for good and sufficient reasons which are not relevant to this discussion to strengthen the rules regarding conflict of interest. This by itself might not have seriously affected matters if simultaneously P Street had not increased the legal responsibilities of members so that from an advisory status they passed to that of members of a Board of Directors who had to vote and whose vote was required by the Office of the Chairman NDRC. Under the force of these two circumstances, the persons best qualified to act as members of the Division had to be divorced.

Had the stringent rules regarding the duties of members and conflict of interest been imposed at the beginning of the work of OSRD, it is doubtful whether OSRD could have functioned at all. As it was, the momentum of the organization could survive this serious attack. But in any future organization of the sort, for peace or for war, it is essential to work out a method whereby the best qualified persons can serve their nation without being exposed to the charge of illegality. It will not be sufficient, moreover, to restrict such freedom to technically responsible personnel, because skillful administrators are as hard to find as skillful technicians.

Unlike many Federal agencies, the central administration of OSRD and NDRC operated with a relatively small staff and accomplished a great many of its objectives by decentralizing the responsibility to the Divisions. This placed upon the Division Chief responsibilities of a widely varying nature. It was one thing for the Director of OSRD and the Secretary of War to agree, for example, that there should be perfect liaison between a division of NDRC and a department of the Army and another thing to bring that liaison to fruition. When balked, the Division Chief could scarcely turn to the Director for peremptory action, and in anything but the most extreme and flagrant cases of non-co-operation, he would have to bail himself out.

Following, then, were the principal difficulties which it fell upon the Division administration to resolve as seen from the worm's eye of Division Headquarters: They were the difficulty of obtaining contractors, of obtaining and retaining scientific personnel, of effecting satisfactory liaison especially with the armed services, of overcoming passive or active opposition

by the armed services, of groping in the fog created by excessive compartmentation and security, and of meeting some of the official obligations, imposed by OSRD either on its own or as spokesman for some other government agency. The discussion here will define the problem and will give examples of what might be involved but will attempt neither to give every instance of a specific difficulty or even most of them, nor to pick out with care the most dramatic examples for treatment. Rather the examples chosen have been selected to be as typical as possible so that the picture may not be painted from too lurid a palette.[2]

## Securing Contractors

OSRD depended on a system of contracts with laboratories, industrial, educational and governmental, for the undertaking of research. Those familiar with the methodology of research will realize that, though the general objectives can be specified (and in wartime must be specified), any effort to hold the experimenter too closely to a rigorously foreordained program will usually not only result in failure to achieve the primary objective but will also inhibit the unexpected and collateral results which are often the finest fruit of research. Consequently, general policy was to drive with as loose a rein as possible. Now driving with a loose rein implies in turn that the horse is an able horse. Consequently, the primary consideration in selecting any contractor for research was the availability in the contractor's laboratories of top-notch men in the field of interest. It would then be expected that the original group would be able to collect around them, from far-off sources if need be, the additional people necessary to carry the project to a successful conclusion. Having then selected an institution which seemed likely to contribute to the project in hand because of specialized personnel, equipment, or experience, and because of clearly adequate responsibility, it remained necessary to persuade the contractor that he ought, as a matter of national duty, to undertake the work. Thanks to the careful way in which the Contract Section of the central OSRD office had worked out standard contract details, and despite objections to the patent clause in a very few instances, no laboratory refused a Division 2 contract through dissatisfaction with its terms. The crux always lay in the contractor's estimate of his capacity to perform well. Until almost the end of its contract negotiations, Division 2's selection of contractors was based upon the presence in the contractor's organization of a key man. Thus, of the twenty-six contracts sponsored by the Division, fifteen were made with institutions because of the presence there of a man or group of men who were distinguished in the field of interest; another six were made as natural follow-ups with some of the institutions in the first group.

[2] A specially dramatic case, illustrating almost all the difficulties which were overcome on the way to success, is that of the Frangible Bullet which is described in detail in the following chapter and hence is not mentioned here.

In the very early days of NDRC an *ad hoc* Armor Committee[3] was appointed to make a survey of armor research in all federal agencies, and to recommend what, if anything, NDRC should do in this field. Armor research had, of course, always been a primary prerogative of the Services and especially of the Ordnance Departments of the Army and the Navy, which at this stage of the war, anyway, had scant enthusiasm for the help which might be given by a bunch of scientists untutored in the totems and taboos of ballistics. When the Committee completed its review, the decision was made that NDRC should continue its work on the more fundamental problems. One of these dealt with behavior of armor under high pressures; in this field Bridgman of Harvard University had no peer and Harvard was naturally selected as the contractor. A second dealt with the effect of rapid loading on strain patterns, and here Seitz of the University of Pennsylvania was well known and had already had some experimental contact with the Services.[4] The same pattern could be found throughout this process of selection; at the California Institute of Technology there was von Karman, a most distinguished aeronautical engineer and as well equipped as anyone in the country to deal with the supersonic wind tunnel program which was commenced there; at the Polaroid Corporation was Land and a group of colleagues[5] who had made a substantial reputation for versatility and imagination in the use of plastics, and it was natural that they should be selected to study synthetic substitutes for armor; at the University of Illinois Richart and Newmark were famous for their advanced work in concrete and were the obvious people to undertake work on the effect of impact on individual concrete members; Wilbur at the Massachusetts Institute of Technology was well known among civil engineers for his structures laboratory and thus deemed well fitted to undertake model studies of the vibration of structures under impact; when the rapid-rates-of-strain project was refereed by a special Committee headed by Robertson, the most obvious choices for further work in this field lay between von Karman at California Technology, Nadai at Westinghouse Electric and Manufacturing Company, de Forest at M.I.T. Generally speaking, where a man was approached because of his status as an expert, it followed that he could readily be interested in the project; when this interest was aroused he, in turn, was of assistance in persuading his employer to undertake the contract, for the employer had the assurance from within his own organization that there

[3]The Committee consisted of four prominent metallurgists familiar with armor problems because of their association with producers, one representative each of the Army and Navy, and one member (Bleakney) from the division.

[4]The case was so clear here that when Seitz resigned from the University of Pennsylvania in November 1942 to become Head of the Department of Physics at Carnegie Institute of Technology, the contract with Pennsylvania was terminated, the apparatus moved to Carnegie and a new contract written with that institution.

[5]Including Dr. B. H. Billings, O. E. Wolff and F. J. Benda.

was a chance to make a successful research and useful contribution. This type of contract, once the choice had been made, was therefore quite easy to negotiate.

This inevitably led to the next stage. In at least some of the institutions already under contract, the field of research began to widen. At this time the question always arose whether a new contractor should be sought or the duties of the old one expanded. Here the political climate of the nation and of the institutions was of importance. As a generalization, the privately endowed institutions or the private corporations were more freely able to enter into contracts, certainly before Pearl Harbor and to a considerable extent for some months after, than were those dependent upon state funds and therefore subject to state controls. It was even hard in those early days to persuade institutions in the center of the country, even when not bound by such restrictions, that the time had come for them to devote their research resources wholeheartedly to the progress of the war.[6] Under these circumstances, it was inevitable that individual scientists at such institutions became restive and, with the consent of their administrations, departed in considerable number on loan to the institutions which had entered the field in advance of the national, or at least of the local, public opinion. When the political opinion had become favorable, many of the institutions were so depleted of top men that they were no longer logical choices for the work.[7] Under these circumstances, it was usually easier to enlarge the scope of activity of an existing contractor.

As the war went on, the reservoir of unoccupied scientists was drained dry and the existing contractors could take on new problems only at the expense of discontinuing old ones. How serious this situation had become in Division 2 is dramatically illustrated by the case of the muzzle blast program which developed in mid-1944, and which has been described heretofore. This was a high-priority crash program. The Princeton Station, already overloaded, was able from its experience to contribute certain basic information and to seek some more, but it could not make a frontal attack on the problem. In such a crash program what was needed was the simultaneous application of a number of skillful, independent but co-ordinated groups, any one of whom *might* come up with the solution. There were a number of industrial and other laboratories whose general experience could clearly be brought to bear. But it was with the utmost difficulty that

[6]For example, the Chief of Division 2 attempted to persuade a very important association situated in Chicago, which supported superior laboratories in a field of interest, to undertake an important part of the fortification work. This was in 1941. He was unable to interest this group in discontinuing any of their peacetime work, even to the extent of lending a single man, much less to the extent of taking on a project. Some months later when the Selective Service Act began to bite, this same institution was looking for work.

[7]This has led to a good deal of uninformed criticism of NDRC's concentration of contracts.

the Division was able to persuade two of the potential groups to undertake any part of the problem. These organizations simply had no further competent persons to put on the project, and they were properly unwilling to drop what they were doing in favor of the new without a clear priority directive from the Services or the OSRD. Save for the most dramatic endeavors such as that of the Manhattan District, the Services were never able to give such a directive; working to some degree in reflected light, NDRC also could, and did, but rarely take a clear position in these matters.[8]

Within a division, and especially within the program of a single contractor, the divisional administration could, of course, take such a responsibility and frequently had to, but this was always a difficult decision for the Division Head, who was scarcely in position, due to compartmentation and security, to view the war as a whole.

As things turned out, Division 2 was not able to meet all its obligations fully. Had the war continued much longer, this would have become increasingly serious and a stiffer priority policy would have been inevitable.

## Securing and Retaining Personnel

A literal interpretation of the responsibility of a contractor might have discharged the divisional administration from necessity for assisting in the procuring of any scientific personnel other than the few technical aides and other governmental employees needed for the divisional administration itself. In practice, however, this was not the case. The divisional administration was directly concerned with the progress of the work and added its influence as it could for the purpose of increasing the personnel to the limits needed. Moreover, once personnel was obtained, it was necessary to work constructively to retain them. The Division Headquarters was never able to forget the serious shortage which existed in trained personnel, a shortage which grew worse steadily, accented by the increase in the demand for research, the need for trained scientists in the combat areas, and a Selective Service policy which destroyed the seeds of any new supply from the very first day it was put into effect.

Most of the Division 2 contracts were relatively small, and the contractors could rely on the handful of individuals already on their staffs to carry out the work. This was not the case with two prominent contractors, the Woods Hole Oceanographic Institution and the Princeton University

[8]This would have been a matter of special difficulty for the decision would not ordinarily have had to be between say two matters of ordnance but rather between a ground weapon and an air weapon or indeed between two things which would be still less comparable. The proponents for priority for each project would have come from different divisions and have had no adequate meeting ground for debate. The refereeing would have been almost impossible. This probably as much as anything dictated the generally laissez-faire policy in which review, suggestion and guidance were substituted for mandate. On the other hand, the Division Chief clearly could not be wise where his superiors felt unable to be.

Station. Although in the last analysis any personnel director relies on the opinion of those he trusts in the selection of new personnel, there can be differences in the ways in which the personnel are first recruited. A large institution like the Radiation Laboratory at M.I.T., for example, could never have put together 1800-odd technicians of various degrees of skill without wholesale recruiting in the highways and byways. This the Radiation Laboratory indulged in early and, no doubt, their aggressive and competent scouring of the country shortened the supply materially. Such a large job as that undertaken by the Radiation Laboratory involved a very substantial proportion of engineering and development as opposed to research. Under these circumstances, it was possible to graft a large organization of generally competent persons of various training on to a nucleus of top-flight scientists. On the other hand, a Division which was engaged almost exclusively in research could conduct its affairs only with persons who were skilled in research and had to limit its personnel procurement policy accordingly.

Princeton University and Woods Hole had then to follow a different and more personal policy. For example, almost every key person on the staff of the Princeton Station was there because he was well known previously either to Bleakney, Robertson, Beckwith or Burchard. The process inevitably restricted the total number who could be mustered and to that extent limited the scope of the work which could be undertaken; on the other hand the cohesion of the group could be relatively great and could result in some increase in efficiency.

Princeton, therefore, for some time did not deem it necessary to go out on its own to find additional individuals. Instead, it relied on the agencies which had been established expressly for the purpose of assisting in the procurement of personnel. These were the United States Employment Services, the OSRD Scientific Personnel Office, and the National Roster of Scientific and Specialized Personnel. Not one of these agencies succeeded in presenting to the management of Princeton a candidate who later turned out to play any sort of significant role in the research which was conducted.[9] Thus, Princeton was left rather squarely on its own.[10] The first procure-

[9]This was not unnatural. Most good men were well known to some project supervisor and had been snapped up before the roster people had issued their questionnaires.

[10]The principal personnel were mustered as follows: R. A. Beth, Ph.D. at Frankfurt, Germany, 1932, New York-born, had been a research associate at Princeton in 1934–35 and was Associate Professor of Applied Mathematics at Michigan State College at the time when his institution loaned him to Princeton for the duration; H. F. Bohnenblust, Swiss-born and -educated, had taken his Ph.D. at Princeton in 1931 and was Associate Professor of Mathematics at Princeton at the beginning of the project; C. W. Curtis, Ph.D. at Princeton in 1936, was Assistant Professor of Physics at Western Reserve University when the war began; L. A. Delsasso, Ph.D. from California Institute of Technology, was instructor of Physics at Princeton when he turned to Division 2 work; C. W. Lampson had taken his Ph.D. at Princeton in 1937 and was Assistant Professor of Physics at University of Richmond when he was invited to Princeton; V. Rojansky, Professor of

ment was none too hard because it was largely done in the early months of the war. It became increasingly difficult later, to the point of impossibility. The fight now was to retain them and that was a harder fight than that of getting them.

The forces tending to draw men away from the work of Division 2 were in ascending order of strength:

(a) Competition from industry
(b) Competition from other NDRC projects
(c) Competition from Service laboratories
(d) The Selective Service
(e) Competition from Operational Research Groups.

Industrial competition never proved to be a serious factor. The OSRD pay formula was relatively fair and resulted in adequate compensation for the contract personnel. Nonetheless, it did not raise the salary levels to the point where industry could not easily outbid the university contractor. There were regulations of various sorts designed to prevent changing jobs for higher pay, and capable in theory of completely preventing this in the case of men of draft age. In practice, if a man really wanted to go and if the other job were a war job, it was pointless to forbid him, as he could not be expected thereafter to make a worth-while contribution to his project. It is greatly to the credit of the assembled group that they considered the work they were doing of such importance and their loyalty to their first employer so significant that no man of any consequence was ever lost to this competition.

Competition from other NDRC projects was perpetual but not forceful. The directors of other projects were debarred from discussing a new job with a specific man without the consent of his present employer. This was

Physics at Union College, had been a National Research Fellow at Princeton in 1931–32 and a visiting Associate Professor there in 1941–42; H. Scheffé, Ph.D. at Wisconsin, was teaching Mathematics at Princeton in 1941 when he was drawn into the project; L. G. Smith had taken his Ph.D. in Physics at Princeton in 1937 and was on the staff there at the outbreak of the war; A. H. Taub had been a fellow at Princeton 1932–34 and had taken his Ph.D. there in 1935. He was Assistant Professor of Mathematics at University of Washington (Seattle) when he was loaned to Princeton.

G. T. Reynolds, J. G. Stipe, R. J. Slutz (who later became Technical Aide to the Division), R. L. Kramer, and J. D. Pittenger were all students in residence at Princeton, candidates for advanced degrees. C. H. Fletcher had been associated with Curtis at Western Reserve. Pittenger, Stoner and Emrich were Princeton graduates and were drawn back from widely scattered points.

David Mayer, graduate of Harvard, had been associated with Burchard at M.I.T.; R. J. Hansen, graduate of University of Washington, had matriculated for an advanced degree at M.I.T. and came to Princeton on the recommendation of Professor Babcock; N. C. Dahl, also a graduate of University of Washington, was procured on the recommendation of Dean Loew when he was asked by Burchard to supply another Hansen. Of these principal engineers, only D. G. Kretsinger, who was loaned by the U.S. Bureau of Reclamation, and M. E. DeReus, who was loaned by Kansas State Highway Commission, were obtained by Burchard as shots in the dark and on the recommendation of persons whom he also did not know but who had substantial official positions.

almost always lived up to. In the event, which was usual, that the present employer refused his consent, the new bidder might appeal to the Chairman of NDRC. This office usually avoided intervening and never in the case of Division 2 issued an actual instruction that conversations should be held. At various times the Radiation Laboratory and the Manhattan District sought some of the key men, the former soliciting even Bleakney, at an early stage without consulting Tolman or anyone else. The upshot of all this was negligible save in the confusion it created. From its point of view, the division would have welcomed a more arbitrary and decisive position on the part of the NDRC with respect to the priority of demands for scientific personnel.

Competition from the Service laboratories was unremitting and increased with time. This was occasionally offered as a formal matter but usually informally. The individual representatives of the Services, acting presumably without a formal official blessing, paid less attention to the regulations and frequently discussed the possibility with the man before they asked permission of the Division Chief or the head of the contractor's organization. By mutual agreement Delsasso left Princeton Station in January 1943 to join the staff of the Aberdeen Proving Ground. Otherwise, the Division lost no key person to any Service research unit. The workers in the NDRC projects were thoroughly familiar with the way Service research was organized and the position of the civilian in the whole Service scheme. The conditions in NDRC appealed to these workers universally, as so much more favorable to good scientific work, that so long as NDRC projects appeared equally exciting they were under no impulse to change their base. This is an important point which is worth pondering in the organization of future military scientific research or, indeed, government scientific research in general.

The Selective Service created a great deal of difficulty. Actually the record of the division in loss of men to the armed forces by draft was extremely good. What was serious was that Selective Service constantly shifted the rules, or at least the interpretation of the rules, apparently under various political pressures as they arose, and always publicized these shifts widely. Every time this happened a wave of emotion rolled through the junior personnel and matters were upset for some weeks. Frequently some senior man had to talk to each of the younger men before the wave abated. The men were not afraid of being drafted *per se;* indeed, they usually felt that each new interpretation challenged them to decide whether their government really considered them as helpful to the war as they would be on the combat front, where, no matter how much less they might achieve, they could be sure of public approval. An equally nagging result of the varying regulations of the Selective Service system was the long battle which had to be waged to keep technical men when they were not actually employed with a slide rule or at a bench. The Selective Service authorities never

seemed to realize that many administrative acts of such an organization as NDRC required technical training and could not be handled by superannuated scientists or green girls. The administration of Division 2 can see no reason to forgive Selective Service for its wavering policy. Selective Service actually made almost no inroads on the personnel or anything other than the temper of the division, but this strain was constant and wearing.

Another very insidious effect of Selective Service was that at times it actually stopped the prosecution of projects. There was, for example, a period when deferred personnel could not work on a project which had not been formally endorsed by the Army or Navy. Since many of the most successful NDRC projects were, at least in their early stages, of this nature, this was evidently a step backward. After months of effort the Director OSRD was given the power to make this formal endorsement on his own account. In the interim, the practical result was that no new project without endorsement could readily be undertaken. In Division 2 this specifically deferred work on the Frangible Bullet project for many months.

A more serious but less obvious effect of Selective Service, however, was the set of rulings which effectively stopped the training of young scientists and engineers. It will be realized that the war lasted long enough so that had these persons been exempt while in training we would have had a whole new college generation of men ready to add to the depleted rosters of scientific personnel. This was the major stupidity of the draft administration and one not duplicated so far as is known in any other country, unless it were the enemy countries.

All told, the administration of Division 2 has no occasion to respect the policies or the administration of the top levels of Selective Service although the local boards exercised a saving common sense; or for that matter the pusillanimous attitude of the Services with respect to their own civilian personnel and the draft. This attitude by the War and Navy Departments certainly perpetuated policies of Selective Service which would have been abandoned had either of the Services taken a strong position. A great deal of credit is due to the few fearless men in OSRD who fought tirelessly for the necessary scientific manpower, and who, in the face of a general political manhandling of manpower problems, won astounding concessions.

By far the greatest pressure on personnel, felt with increasing force as the war went on, was that exerted by the various groups whose business it was to place men of scientific training near to the fighting fronts and in contact with operational as opposed to laboratory problems. It was inevitable and right that such groups should exist. As the entire history of the Office of Field Service, for example, will show,[11] there was an extensive need for such service, both from those divisions which were making new apparatus and from an information division such as Division 2; such a division could

[11]See the volume *Combat Scientists.*

not be fully effective until its information reached the fighting forces and was properly understood, interpreted, and used there. In addition, service with the fighting forces had a strong emotional appeal, especially for the younger men, who could thereby earn at least some small part of the public esteem understandably reserved for those exposed to actual physical injury and death.

The first inkling that this might become a major administrative problem for the Division came in October 1942 when Robertson was invited to go to the United Kingdom to become part of the first Operational Research Section established by a field force of the United States, the 8th Bomber Command. Since Robertson had already vitally interested himself in operational research and since this represented precisely the sort of job which he thought needed to be done, it is not surprising that he wanted to go. The division resisted vigorously the loss of such a key man. Nonetheless, Robertson finally determined that he must go, and in the light of the end event, this proved to be a right decision. His experience with the 8th Bomber Command led to improvement in the situation there and better work in subsequent operational research sections and was the beginning of the distinguished career he subsequently pursued in the ETO, which does not belong in this particular account.

Negotiations for his first mission had been conducted directly with Robertson by Major W. Barton Leach, AAF. It was apparent to the Division that this was but the first of many forays by an expanding Air Force, for Division 2 was almost the only place in the United States to which the Air Forces could turn for men skilled in forecasting the effects of bombing. Yet the Station could afford no further loss of its principal scientists. Accordingly, Burchard approached Major Leach and proposed that Princeton recruit and train a special cadre of new men, and that as the Air Forces required more personnel, it would come directly and only to Division Headquarters and take the men recommended by the division. This agreement was faithfully kept by both sides. The division selected, trained and delivered to the Air Forces several high-grade groups of young architects and engineers who were ultimately associated with Air Force operational units from India and China through the Central Pacific and the Aleutians to England, Africa and the Mediterranean. Nevertheless, there continued to be sporadic losses of personnel to the Services.[12] These were straws in the wind.

Meanwhile, Burchard was himself becoming more concerned with opera-

[12]For example, Mayer, having finished his project on earth shock in Oklahoma in early 1944, was released at his own request to undertake operational research with the AAF in the China-Burma-India Theater; Reynolds at Princeton, on his own application, was released and in mid-1944 assisted in obtaining a commission as ensign U.S.N.R. Brothers, assistant to Bowman at Drexel Institute, after a tour of duty for Princeton in England, was also granted release to serve with the Air Forces, which he first served in India and ultimately as Chief of the Operational Research Section of the 20th Bomber Command.

tional matters. In the spring of 1943 he had been made Chairman of an *ad hoc* committee dealing with navigational aids to landing operations for the Commander in Chief, U.S. Fleet. This operational problem drew increasing amounts of his time so that he often was absent at sea or at naval stations in New England, Norfolk, or Florida for some weeks at a time. In the fall of the same year, he was made Chairman of a second amphibious landing committee dealing with the demolition of obstacles to landing operations. This meant that in the months of January and February 1944 he was absent all the time at Fort Pierce, Florida, or in the Caribbean, and by May 1944 he had spent another month at San Diego. In June he departed for service on the staff of Lieutenant General Richardson, Commanding General, POA, and with this assignment he resigned as Chief of the Division[13] to become Assistant Chief of the Office of Field Service, of which the primary responsibility was to recruit trained men for service with the active commands abroad.

The Office of Field Service proved a formidable new competitor for men. It had the blessing of the Director, OSRD. Its Chief, K. T. Compton, its Deputy Chief, Waterman, and its Assistant Chiefs, Harrison, Klopsteg, Morse, Tate and Burchard, had all had long experience with NDRC and knew the qualities and the interests of many individual workers. With the change in the course of the war, with the inside knowledge which the former Chief of Division 2 had about his former Division, it was inevitable that the pressures should be high and often irresistible. The temper of the younger men was surely changing. For example, project AN-23, although administered by the Division, was really an operational problem and effectively removed three key men, Bohnenblust, Rojansky and Hansen, from participation in the Princeton research program. Dahl of Princeton and Newmark of Illinois were anxious to get into more active areas and were ultimately turned over to OFS, which stationed them in the command of General Richardson. Later, Dahl went to Guam for the 20th Air Force. White, because of his presence in the London Mission at the time of D–Day, had been given early experience in operations on the continent. Although not formally released until June 1945, he was thereafter clearly lost to the Division. When the Joint Target Group was formed by the Joint Chiefs of Staff to study the operations of the aerial bombardment of Japan and sought through OFS a civilian adviser on physical damage, it was inevitable that White should be approached and accept. In November of 1944 Kelly resigned as Special Assistant to the Chief to join the ALSOS Mission, an OFS scientific intelligence mission on the continent of Europe.

[13]The operations did not involve Division 2 appreciably save for the time drain they laid on the Chief and the corresponding additional load thus placed on Bleakney, the Deputy Chief. Bleakney was a member of the demolition of obstacles committee and Beth worked for this committee for two months in the winter 1943–44. The history of the operations will be found in the volume *Combat Scientists*.

In July 1945 Beth was detached from Princeton to become a member of this same mission. In July 1945 Bleakney himself joined the ALSOS Mission for a short tour of duty.

The breach had been opened and had the Japanese war persisted much longer it would have been hard to hold it. Nonetheless, it had been held in the critical times, and by the time the emphasis shifted to operational work, it was probably entirely appropriate that it should.

All told, approximately one-third of the senior personnel of Division 2 was ultimately lost to this field type of activity, a sensible balance between the needs of the laboratory to cover an event six months or more away and the needs of the field to cover an event tomorrow.

## Problems of Co-operation

NDRC success depended entirely upon a satisfactory basis of co-operation with the Services. In the final analysis, all good work would go for naught if somewhere in the armed forces there were not someone to carry on; as gear was useless without full procurement and field use, so was information wasted unless the man making the battlefield decisions had it, had confidence in it and knew how to use it.

For Division 2, as an information division, there was an additional problem. The man who had designed a piece of good apparatus could (and often did) arrange a demonstration despite all opposition. Even if no one had previously bothered to read his progress reports, still he could get important people to look at his new apparatus in operation. If the demonstration were successful, the way could be cleared to get it into war use, and he might even be asked to write the Service manual for it and to train the men to operate it. Such was the experience with radar, with rockets, with amphibious vehicles, with amphibious navigation. The road of the purveyor of information was not quite so direct. Although the apparatus man was treading close to the preserves of the procurement bureaus, the information man was approaching holier ground, the field of operational command. He had to step with constant care.

The natural field of interest for Division 2 was ordnance. Ordnance is, traditionally for both Navy and Army and especially for the latter, one of the oldest technical branches, the most self-contained by tradition, the most sure of itself, and the most obdurate in maintenance of its prestige.[14] This tradition could not fail to make itself felt in its dealings with the scientific upstarts who offered the further affront that they did not start (as would have been the case in electronics, nuclear physics, and many other fields) with an admittedly greater technical knowledge in the subject field than the officers and civilians of Ordnance could claim for themselves.[15]

[14] This is as well understood by officers in other branches and bureaus as by civilians.
[15] It is true, however, that the basic equipment of the NDRC personnel was superior

The story of the efforts of Division 2 to establish a going basis of co-operation epitomizes the difficulties to be encountered by any outside research group coming into contact with a working group with a long tradition, and is therefore of historical importance.

The general impression of the division throughout the war was one of constantly trying to break through a barrier against co-operative effort. At the outset this barrier was frankly aggressive and bitter in its opposition; with time it became subtler and relied upon military delay, "buckpassing," and other less obvious devices; it constantly but slowly diminished as people got to know each other better and as recalcitrant officers were retired or replaced; and it might have disappeared if the war had lasted longer. A start made on the basis of the relations prevailing towards the end of the war would unquestionably have produced a far more fruitful five years.

The fault was, of course, mutual. With the NDRC personnel, it manifested itself in impatience with military procedures, in an unwillingness to understand their purpose, and even in an inclination to believe that all "channels" were *ipso facto* stuffy and therefore to be detoured. This resulted in conduct which often seemed brash, and occasionally worse, to the military.[16] With the Services, the fault manifested itself in a reluctance to use anything developed by these outside civilians, and in a preference to accept help only on the basis of a complete subservience on the part of NDRC.[17] When that failed, there was a desire to restrict the activities of the OSRD group to small areas considered "harmless."

These were the intuitive positions. The OSRD people underrated the intelligence of the Service people; the Service people overestimated the desire of OSRD to "nose in everywhere." With the passage of time, thought replaced intuition. Had this not been so, the progress could not have been so substantial as it was.[18]

We have remarked upon the general impression of opposition. There were very few open clashes, however, especially after the first months. The situation was rather one of atmosphere than of daily trouble. To an outsider the opposition seemed to stem from three principal factors:

to that of all but a few officers and civilians in Ordnance, which had been starved of its technically trained personnel for years, both through lack of funds and through a general policy common to both Army and Navy which sharply limited the promotional possibilities of those who neglected operational matters in favor of research.

16These head-on collisions should have been reserved for basic issues in which important opposition had to be overcome.

17Which would have been quite fatal. It was not until 1944, however, that efforts were given up, for example, to transfer the Princeton staff almost bodily to the Aberdeen Proving Ground.

18This account has stressed relations with Ordnance of both Services. Division 2 had projects with many other branches, including the operating command as well. The first relations, with the Corps of Engineers, were universally satisfactory. The other most important relations were with the Air Force, at once the most impetuous and the most tolerant of the Service groups, the most avid for help of all kinds, and the most curious about all developments, as was natural for a new, energetic and ambitious Service in itself regarded as "brash" by its military colleagues, even to the matter of its headgear.

(a) The very natural reaction of any group of specialists who had made sacrifices for years, who had done the best they could with inadequate financial and other resources, and who now saw their position threatened and inexperienced outsiders endowed with funds and power beyond their own wildest dreams.
(b) The fear on the part of a few older officers who had traditionally fought new developments that this group of uninhibited scientists would tend to place them in a position of personal blame.
(c) The feeling of some officers that the NDRC group really could offer nothing useful unless it accepted more direction than it was willing to accept.

The first intimation that Division 2 might meet this sort of difficulty occurred in 1940 when Robertson and Burchard, accompanied by Mr. (later Lieutenant Colonel) Sherwood B. Smith, called upon a then well-known Ordnance colonel to request help. This colonel, long since retired, simply exploded. He was rude; he was apoplectic; he launched a tirade. Tempers grew short on both sides. Finally, the civilians said they would handle the matter themselves, no matter how difficult. At this point, the colonel asked Smith if the Chief of Engineers wished the work done. When informed that this was the case, he changed and said calmly, "Very well, gentlemen, the Ordnance Department will comply with the desires of *the Chief of Engineers.*"

As time went on such men usually disappeared. Opposition changed from the combative type to passive resistance or to none. Sometimes no apparent reason for opposition existed. For example, early relations with the Naval Proving Ground at Dahlgren, Virginia, were remarkably good. Introductions had been made by Captain Trexel, BuDocks, who was conducting tests on bombproofs at which Bleakney, Robertson and others were observers. Close working arrangements were established with the proof officer, Lieutenant Commander (later Rear Admiral) Parsons, and with the principal civilian, Dr. L. T. E. Thompson. Ultimately, these two men both left for other duty and a new commanding officer also came to Dahlgren. It then became increasingly difficult to exchange information with this important station. Officers and civilians of the command who wished to attend meetings sponsored by the Division were not permitted to come; an effort to arrange a meeting at Dahlgren was a failure. The reasons for this change of attitude have never been clear to the Division 2 administration.

Substantial but indirect opposition was encountered in connection with the gun blast program. Here the Service attitude was that the matter was well in hand [19] and that Division 2 could not make a serious contribution unless in the area of pure theory. This ran so deep that the NDRC had to

[19] Which it certainly was not.

alter its division assignments and the division had to be careful in selecting its contractors, so that groups in the good graces of Ordnance would not prejudice their position by undertaking such unpopular work. An even more serious case was that of the frangible bullet, where ill-considered opposition led to a postponement of reduction to field use for many months.[20]

Much more difficulty arose, not in such sharp incidents as these, but in the general attitudes of the Services toward the work of outsiders. Some of these were funny, some were not. All were, in the light of human nature, understandable.

At times the division would come upon information which the Services did not wish published even within the security zone. For example, a very early set of tests of concrete structures conducted by the Engineers at Edgewood Arsenal involved the dropping of various General Purpose Bombs of the then design on various concrete burster slabs. Observation and photographs revealed that a very large number of the bomb cases deformed when dropped inert. Considerable difficulties and delay were encountered in getting permission to make the information available to the British, who had a scientific interest in it, although in the end they were allowed to look at the photographs. Great harm might have resulted from this unwillingness to profit fully from errors.

Honest differences of opinion could exist as to the desirability of publishing information. Such a case arose in connection with the Divisional Data Sheets. Here, with the co-operation of the Ordnance Department, which furnished a very large amount of key data, the Division was preparing and distributing to a wide list of officers and scientists, many of them in field posts, sets of ballistic curves for various projectiles. It happened that one projectile, which was not in general procurement for other reasons, had a very attractive ballistic performance curve. It is a canon of the War Department, the virtue of which has been vigorously debated by civilians, that field forces shall not be given information about weapons which either are not in procurement or cannot be supplied to a particular theater because of commitments elsewhere. In accordance with this tenet, the Ordnance Department feared that appearance of these data in theaters would lead to requests for a projectile which could not be supplied and that the refusal to supply would lead to misunderstanding and confusion. On the other hand, the division's attitude was that the information was applicable to the general problem which the particular sheets were covering, that it would be used by engineers who had no relation to the procurement of projectiles or even the use of this type of projectile and that more good than harm would be done by circulation of the information. In this case, compromise was possible. The Ordnance Department waived its right

[20]This project is so well illustrative of so many difficulties that it is reserved for a separate chapter.

to forbid the circulation of the information and let the sheet go out with the projectile in question clearly indicated as experimental only.

More often than not the apparent reluctance of the Services to seek fullest co-operation was a matter of preoccupation or indifference rather than of veiled opposition. For example, when the great explosion occurred at Norfolk in September 1943, it did not occur to the Navy to request the admitted experts on damage working for Division 2 for assistance in evaluating the physical effects of the disaster. Yet such an evaluation was of great concern to the many groups then interested in larger bombs and far more powerful explosives, who leaped for every piece of data however fragmentary which would or might bear on the question. When Burchard asked permission to send Bowman to make such an assessment, it was not only readily granted but Bowman was provided with every facility including observation aircraft, photographers, and guides. His report was distributed by the Navy. Yet, when the even greater explosion occurred some months later at Port Chicago (July 1944), again the matter had to be called to the attention of the Navy, which was again very co-operative, this time with Burchard, who made the survey en route home from the Pacific.[21]

More serious than this was the tendency of many responsible officers in the Services to regard NDRC projects as of the type in which the information should flow only one way. A particularly cogent example of this occurred in the case of the high-velocity work, on which Curtis made regular monthly reports of his progress to one section of Ordnance. When the mammoth German tanks were found to be more heavily armored than had been suspected, and an urgent appeal for higher-velocity antitank projectiles was cabled from our forces in France, another section of Ordnance undertook a crash program, crowding other work off the Proving Grounds, spending millions of dollars, and in a few short weeks getting a fair compromise weapon to our antitank gunners. It was a good job, but it seems reasonable to suppose that it might have been more quickly and possibly better done if the group carrying out the project had ever been informed of Curtis's work. The division learned of this crash program when it had ended, although Wilson and Curtis had a conference during this period with the Ordnance representative responsible for liaison on the project.

Military methodology was also sometimes hampering, although this was usually more amusing than anything else. For example, at the beginning of the very extensive tests at Aberdeen on the detailed effects of projectiles striking concrete targets, Beth was placed in charge. It was of the essence that these tests be conducted scientifically with a full and accurate set of measurements. A number of pieces of equipment measuring transients had to be set up and synchronized, which inevitably took time and patience.

[21] The Navy was, of course, making extensive reports of both incidents but not from this angle.

A young gunnery officer had been assigned to fire the guns. The firing of the first round showed Beth that improvements would have to be made in the measuring arrangements. The gunnery officer was trained in a different school and got off several unmeasured rounds before he could be stopped.[22] This was no tragedy, although a few good targets were spoiled. The officer was simply not a research man. His job was to fire guns. His performance and efficiency would in his book be measured by the number of rounds he could get off in a day. Less understandable was the frequent requirement by those in high command of reports on the progress of research in terms of the per cent completed.[23]

More annoying and perhaps more serious was the desire to limit OSRD work to the matter directly in hand and especially to discourage it from the production of new weapons. For example, rather early in his work on the penetration and perforation of concrete, Beth became convinced that it would be possible to design a projectile, quite different from any existing, which would give a materially better performance against concrete. His ideas were transmitted to Ordnance, which stated that there was no operational need for such a projectile. Much later when enemy materiel began to be captured, it was discovered that the Germans had such a projectile in use. Trials at Aberdeen demonstrated its effectiveness, and so our side put into procurement what was essentially a copy of the German projectile. Division 2 personnel doubtless still believe that, had their early recommendation been followed, a still better projectile might have been available, and earlier.[24]

This seemed to be a standard attitude. For example, the problem of hypervelocity was discussed at length at a meeting Major General G. M. Barnes[25] and his staff held with Division 2 on April 23, 1943, in the Pentagon:

> Beth: But the shatter limit is different. For example, if you put a cap on it, it will prove that you can raise the shatter.
>
> Gen. Barnes: We think so, yes. You mean a different kind of cap than we have now.
>
> Col. Zornig: The design of these projectiles has been troubled for years with lack of suitable theory, and I think it is the prime function of this group

[22]Relying in his view on the fact that the powder charge would predict the velocity. Adequate for combat, this criterion was far from satisfactory for scientific measurements.

[23]The humor and the innate impossibility of such a determination will perhaps be fully apparent only to those who are familiar with the processes of research.

[24]The division administration cannot escape blame here. Despite the difficulties of working at large calibers against Service opposition, especially in view of the absence of available proving grounds and guns, the division should probably have fought the matter through to a demonstration.

[25]Chief of the Technical Division, Ordnance Department.

to develop the suitable theory that the Ordnance Department designers could work into ammunition. I don't believe it's practical at this late date to try to train everybody in this group with all the things that have to be considered in designing a piece of ammunition. I think if this group can work out the theories, then the Ordnance office can apply those in a design. That's my own feeling about it.

Gen. Barnes: I think you could help us a great deal if you could nail this theory for us, which we've never had.

With this view both Bleakney and Robertson concurred. It was unquestionably a correct view in the particular instance, in view of the already overloaded state of the facilities at Princeton and elsewhere, and the elementary state of cap research. Had the scientists possessed facts proving the need of a specific weapon, however, as much as two years' delay could be expected for reduction by the Services to production—unless an urgent request intervened. Design by those who developed the theory could save a great deal of time.

The Services could move fast and co-operatively when matters were presented properly and high enough up. Thus, on one occasion, a pointed footnote by Bridgman in a division report brought forth ire and action. It had long been a practice in testing armor to return rejected plate to the manufacturers, who kept to themselves whatever they found out about the reasons for failure. Bridgman was attempting the correlation of high-pressure properties with ballistic performance. In a progress report he remarked that he had never been able to get samples of bad plate for his tests but only good ones. This report ultimately came to the attention of Admiral Blandy, who corrected the situation forthwith by causing bad plates to be sent.

A final and most discouraging attitude, possibly initiated by the Services but embraced all too enthusiastically by NDRC, was that which led to excessive compartmentation of information on the grounds of security. It was rather fantastic for the members of a division who knew so much about the defensive properties of concrete and steel to be led in an intellectual blindfold past an area in which shaped charges were being tested for performance against concrete; to be kept for a long time from knowing the results of projectile firings against mock-ups of German pill-boxes.[26] It was like pulling teeth to get the details of a pertinent program in advance. The

[26]Both these tests were conducted at Aberdeen adjacent to a spot where Division 2 work was going on. The same officers of Engineers and Ordnance were involved in both, Division 2 personnel passed the targets daily on their way to work. Indeed Colonel Claudius H. M. Roberts of the Ordnance Department recognized how ridiculous the situation was and personally initiated letters to NDRC requesting that all interested members of Division 2 and 8 be cleared for all ordnance work on shaped charges.

large blast program at Aberdeen was shared with Division 2 only very late in its inception and long after planning was over. But the Services were not unique in this respect. Division 5 personnel worked almost to the point of freezing the sizes of certain guided missiles, relying solely on Navy advice,[27] before a special meeting was held at which Burchard and Robertson were able to point out the requirement of considerably larger weapons for land targets. When Division 11 and the Chemical Warfare Service carried on an elaborate set of incendiary bombing tests on villages, a Division 2 observer was accepted only at the last moment. From the Division view, more harm in arresting research and development was done by this compartmentation of information than could ever have been done by the additional scrap of information the enemy might have picked up by a more general dissemination of knowledge. The Office of the Chairman NDRC formally resisted overlapping information until the end, but the individual scientists tended to break the barriers down informally to the benefit of all. This is, of course, the way the experienced Army or Navy officer had always operated.

Perhaps nothing in the progress of war research was more disturbing to men trained in science and engineering than to watch the wheezy workings of the elaborate machinery which had been set up for liaison. For scientists, the flow of information is well established through the media of the journals, the abstracts and the meetings of the learned societies. A man of any real competence is expected to be well read in his field. He is avid to hear of new papers, to read them, and to discuss them with their authors.

The scientists approached war research in the same spirit. They sought information, but were balked by compartmentation of work, by security regulations, by entirely inadequate methods of reporting, and by the possible natural reluctance of some officers towards passing out some kinds of important information to any civilian. They sought to give information, but were thwarted because military men largely did not have time to read or even to talk things over.

There were, of course, brilliant exceptions to this inadequate meeting of the military and the civilian scientific mind. In both Services there were men, who were of an inquiring and scientific turn of mind, who did not find the style of the scientists impenetrable and fatiguing, who commented freely on what they read and added their own contributions. Within the experience of Division 2 alone there were many such men.[28] But they

[27] Which was concerned with ship targets and not heavy fortifications, at least at the time.

[28] The following men may be cited for their friendship and aid to the general work of the division. Most recent rank is indicated when known; many of these men have undoubtedly received further promotion. Many others, whose help on specific Division projects was invaluable, have been named in the discussion of the project in question and are therefore not included here.

*ARMY*

*War Department Liaison Office*

Col. R. M. Osborne
Lt. Col. W. P. Allis
Maj. H. E. Noble
Maj. H. W. Miller
Maj. R. J. Powers
Capt. L. I. Crews

*Corps of Engineers*

Col. George Mayo
Col. F. J. Wilson
Col. Cabell Gwathmey
Lt. Col. S. B. Smith
Christian Beck
B. L. Krause

*Camp A. P. Hill*

Capt. T. F. Adams

*Air Forces*

Brig. Gen. R. C. Coupland
Col. C. G. Williamson
Col. W. B. Leach
Col. J. M. Gruitch
Maj. G. Weinbrenner
Maj. O. W. Hammonds
Leroy Brothers

*Ground Forces*

Col. G. M. Dean

*Secretary of War*

E. M. Bowles
R. D. Huntoon

*Ordnance Department*

Maj. Gen. C. C. Williams
Col. C. H. M. Roberts
Col. I. A. Luke
Col. Scott B. Ritchie
Maj. H. A. Ellison

*Jefferson Proving Ground*

Col. W. B. Hardigg

*Aberdeen Proving Ground*

Col. L. E. Simon
Ugo Fano
T. H. Johnson
R. G. Sachs
R. H. Kent

*Watertown Arsenal*

Col. H. H. Zornig
Capt. J. H. Hollomon
C. Zener

*Frankford Arsenal*

Lt. Col. C. H. Greenall
Herschel Smith

*Picatinny Arsenal*

Maj. J. W. Givens

*NAVY*

*Co-ordinator of Research and Development*

Rear Adm. J. A. Furer
Capt. Lybrand Smith
Capt. R. C. Conrad
Comdr. H. G. Dyke
Comdr. B. S. Old
Comdr. T. C. Wilson
Lt. Comdr. J. H. Wakelin, Jr.

*Bureau of Ordnance*

Comdr. Stephen Brunauer
Lt. Comdr. E. N. Ohl
G. K. Hartmann
R. J. Seeger

*Dahlgren Proving Ground*

Rear Adm. W. S. Parsons
Comdr. R. A. Sawyer
L. T. E. Thompson

*Chief of Naval Operations*

Comdr. Francis Bitter

*Bomb Disposal School*

Comdr. D. L. Kaufmann

*Naval Research Laboratory*

Ross Gunn

*Bureau of Yards and Docks*

Capt. C. A. Trexel

*Bureau of Ships*

Lt. Comdr. R. W. Goranson

*David Taylor Model Basin*

Capt. W. P. Roop

*JOINT ARMY–NAVY*

*Joint Target Group*

Lt. Comdr. W. W. Timmis, USNR
Lt. Charles J. Hitch, AUS

*Explosives Safety Board*

Col. C. S. Robinson, AUS
Capt. J. R. Gaylor, USN

were never enough to yield a total impression that liaison was a well-meshed and smoothly running affair.[29] It is only fair to the Services to say that their establishments were enormous and had grown too fast, and that entirely within the Services it was perfectly possible to find this same lack of contact and a considerable despair on the part of the officers who were trying to make a real connection of minds.

Liaison broke down for three principal reasons. In the first instance, there were too few men among the regular officers of distinguished scientific attainment and comprehension and these men were overworked. For example, at the meeting of Division 2 with General Barnes in 1943, the General said:

> We've tried honestly to put experienced and effective personnel on these jobs as liaison officers. We can probably have more but I doubt if we could improve the quality. Colonel Zornig is liaison officer on 37 projects. I venture to say that he is more effective from your standpoint than 10 or 15 people who lack the 30 years of background of Ordnance and military research development that Colonel Zornig has had. We haven't too many of these highly experienced and trained technical officers and have to spread them pretty thin sometimes.

The Division representatives would have heartily concurred with General Barnes, but such a backlog of work was more than any officer, even so able a one as Colonel Zornig, could be expected to carry out, especially when, as was usually the case, he also had the direction of an important project of his own.[30]

The second difficulty lay in the poor distribution of reports within the Services. It was disconcerting to find that the Safety and Security Branch of Ordnance had been running important tests dealing with the storage of explosives without access to pertinent Division 2 data; and to note the relief with which the officer in charge of this project accepted the data when he came upon them quite by accident. It was still more disconcerting to learn that a Division 2-trained man was urging the Air Forces in England to run concrete penetration tests to help determine the chance of successful attack on the submarine pens dotting the French coast. Robertson and Bleakney had already made separate calculations for the 8th Bomber Command based on Princeton data more elaborate than that which would have been obtained from the projected tests, which showed that we could not make an effective perforating attack with available bombs.

This is a general wartime problem. Our personnel had to discover the hard way what experienced officers already knew: that the only kinds of information which travel rapidly in the Services are unwelcome orders and unwritten scuttlebutt.

[29]Except for the smooth operation of the NDRC liaison office.

[30]Colonel Zornig at this time was Director of Research at the Watertown Arsenal, one of the most important research establishments of the Ordnance Department.

Finally, liaison broke down because of insufficient good meetings of the two groups. The meeting with General Barnes and his staff in 1943, previously cited, was a model of its kind. It clarified no end of technical matters, and vastly improved liaison. Meetings like this should have been held at least every three months, but as a point of history this was the only one of its kind for Division 2 in the five years of war.

The division tried its own hand at setting up meetings. First, all project liaison officers were invited to an afternoon session of the divisional meeting transferred to Washington for that purpose. Many came, but it was clear that they did not like having to sit through the discussion of numerous other projects before attention was turned to their own. Subsequently, a number of special conferences were held, on subjects such as armor perforation, hypervelocity, rapid rates of strain, and blast. To these could be invited only the appropriate liaison officers and other interested personnel. These worked out much better but after some time, the interest in these ran out too, and the division fell back on its principal reliance, visits of its own personnel to the offices of particularly friendly and interested officers.

Efforts were made by the top drawers of the Services to improve this situation. The Navy Office of the Co-ordinator of Research and Development, under Rear Admiral J. A. Furer, retained its staff of energetic and co-operative young officers throughout the war. The War Department Liaison Office, on the other hand, had a much broader field to cover and a frequent turnover of personnel. This restricted its activity largely to the processing of papers. Too often, when Division 2 made a good connection, the officer departed for other places. The Co-ordinator, on the other hand, could and did undertake a more fully constructive job, including that of catalysis.

If this account seems to have stressed the bad features of Division 2 relations with the Services, it is because there is more to be learned from what has not worked than from what has. Obviously, there were many cases in which both the liaison and the co-operation were of the highest order. This invariably resulted when the Services had a permanent interest in a particular project of urgency and had a special group looking after it;[31] when Service and Division 2 personnel worked in the same shop;[32] when the Services had actually initiated the project entirely on their own volition;[33]

[31]As the Joint Army-Navy-NDRC Committee on Shaped Charges which sponsored the work of Seitz, Pugh, *et al.*, at Carnegie Institute of Technology on the defense against shaped charges.

[32]As in the case of project AN-23, a joint work by Division 2, Division 11 and Applied Mathematics Panel under V. Rojansky of Division 2 which worked closely with the Air Forces at home and in England in working out the best proportion of incendiary to high-explosive bombs in a bombload.

[33]For example, the work under CE-5 and CE-6 related to the work of the Committees on Passive Protection Against Bombing and Fortification Design with liaison by Colonel F. J. Wilson and Lieutenant Colonel S. B. Smith, both of the Corps of Engineers.

or when there was a close personal relationship on a working basis between an officer at an Army or Navy station and a civilian at a Division 2 station.[34] It also occurred when an officer asked for aid on a specific task under his own jurisdiction[35] or when a part of a larger project employing some Division 2 men got further help from a station of Division 2.[36]

## Problems of Bureaucracy

Any organization so large as OSRD, operating in wartime, is necessarily affected by every change of national policy regarding the control of materials, manpower or other essentials. These policies were usually reflected to the divisions from the Central Office in the form of requests for various kinds of information. Of those which are inherent in operating any business, there is no occasion to speak further. Among the others, however, some warrant at least brief mention.

It was an annoyance to have to pull out every priorities stop in order to procure a single camera, when cameras were known to be wasted by the score among the troops. It was still more annoying, having set up all the necessary paperwork, to find another division taking the camera under a newer and higher priority without warning. It was most annoying to be required to estimate the number of steel balls required many months in the future, when the only known purpose the division had was the sketchy possibility that they might be used in a new plastic armor then under study at small scale. If the experiments were promising, the needs might be great. This led for example to the fantastic estimate of 8,000,000 one-half-inch steel balls for the second quarter of one year. It was aggravating to have to recount the amount of shipments made on behalf of Lend-Lease when the only commodity the division had was information; it was exasperating to have to list all senior and junior scientists in order of indispensability and prepare estimates on how to get along with 20 per cent less; it was nerve-wracking to have to seek Service project numbers and endorsements in order to be able to continue to employ draft-deferred personnel.

No one of these things in itself was bad, save as it manifested to the management of the division the creaking machinery of the upper Government, the veering in policy hither and yon, whether it affected materials,

[34]As for example, the relations between Seitz and Lieutenant Colonel Greenall of Frankford Arsenal in the study of copper crusher gauges for measuring the pressure in gun barrels.

[35]As was the case with Colonel C. G. Williamson, AAF, who asked the Division to make recommendations for the revision of TC 50, an Air Force publication on the selection of the proper bomb against a specified target; and with Commander F. Bitter who obtained similar aid for the Navy in the publication of CNO's Table of Bombs and Fuzes.

[36]E.g. The Joint Target Group of the Joint Chiefs of Staff.

priorities, or the supply of scientific personnel. No one was disposed in thoughtful moments to blame the Central Office of OSRD, which but pushed on to the divisions the ineptitudes of management in other places. Moreover, no one of these aggravations nor all of them *in toto* were insurmountable.

The real point was that, since young administrative personnel were not deferrable and older and competent ones not obtainable, it necessarily fell to the lot of scientists to spend more time than should have been spared in selling projects, in currying friendship with officers and civilians of the Services, in making draft affidavits, in casting up estimates for 8,000,000 steel balls. It certainly cut into the efficiency and performance of the leading scientists of the division to a very substantial extent. All of this may seem obvious to the student of government, obvious and unavoidable. It must be avoided, in part at least, in future essays of the same magnitude.

# CHAPTER XXX

## *THE LITTLE BULLET THAT DIDN'T HURT*

FIRED constantly by a lesser Billy Mitchell, a captain and finally major of the AAF reserve, Cameron Fairchild, who risked rebuke and worse until what he believed was possible came true, and starting from scratch against the tradition of ordnance development, Paul Gross, Marcus Hobbs and their associates at Duke University fought almost by main personal strength a three-year struggle resulting in the frangible bullet technique for training flexible gunners.

The technique as used at war's end consisted in firing a breakable plastic bullet through a modified 0.30-caliber machine gun in a bomber at a target airplane. The target plane and its pilot were protected by light dural armor and bullet-proof glass. The pilot simulated attacks on the bomber; his armor broke up the bullet which flew essentially as would a real one; when it hit the target the armor plate vibrated, the vibration was amplified electrically, a counter registered, a bright light appeared in the nose, the gunner knew he had scored a hit and his hits were automatically counted. It all looks easy now.

In April 1942 Fairchild was the Officer in Charge of Special Projects, Training Division, Harlingen Army Gunnery School, one of several schools started that year by the Air Forces to train flexible gunners for heavy bombers. Like many of his colleagues, Fairchild realized that towed socks, motion-picture gunnery, firing at plane models with BB guns were not adequate simulation of the actual conditions.[1] Unlike most of them, he did something about it. What he did was to write to a hundred universities asking them for ideas and designs. He received replies from eighty. Two were important.

The letter to Duke University was referred to Paul Gross.[2] Subsequent correspondence with Fairchild brought Gross to Harlingen for a three days' stay in June; here he took some training himself, concluded that Fairchild was right, that the training was wrong; here he analyzed the eighty replies; here he became a carrier of the torch.

From Professor A. D. Moore[3] came the suggestion of using a bullet of

[1]None of these could prepare a gunner for the difficulties of firing a movable gun from a speeding bomber against an attacking fighter in the dizzy complexities of high-altitude combat, where no fixed points of reference exist.

[2]Professor of Chemistry, Chairman of the Duke University Research Council.

[3]Of Michigan's Engineering School.

tempered glass similar to a Prince Rupert's drop. In this Gross saw some possibility; and though ultimately the project took a different form, the Prince Rupert's drops which Gross supplied to Fairchild were wonderful selling pieces when broken on the desks of some of the high command.[4]

On return to Duke, and on his own, Gross engaged the interest of Marcus E. Hobbs,[5] who remained an enthusiastic and key collaborator to the end. Together they made on a crude range experimental firings with bullets of solid glass and glass-filled shells; they also made bullets of Bakelite. Both were successfully fired from a .22-caliber rifle and broke on impact on light sheet metal at low velocities which were measured only approximately. All this was encouraging.

By July 1942 Fairchild could write Gross that he thought he had aroused enough interest to warrant taking the project to NDRC; he also wanted to visit glass companies. Gross urged him to keep the project broader than glass. Together on July 15 Gross and Fairchild called on R. C. Tolman,[6] who later in the day introduced them to Burchard, together with their problem: to fire through a standard weapon a projectile with all the standard characteristics save one; it would do no damage. This was simply and obviously impossible, but it clearly had to be done. Perhaps some compromises could be introduced. A conference was arranged with representatives of Division 2[7] for July 16 in Princeton.

Subsequent conferences in early August paved the way for a temporary co-operation. Duke would continue, on its own, to experiment with various materials with help by the Bakelite Corporation, which was also working on a voluntary basis; Princeton would conduct trial firings and assist Duke in establishing its own velocity-measuring devices. Taub and Curtis would make calculations looking to necessary modifications of the sight in view of probable changes in the ballistic characteristics of the bullet. Meanwhile, Division 2 would try to assist Fairchild to procure an official Army project for NDRC, which would make it possible to write contracts with Duke, Bakelite and others and press the work more vigorously.

In the later days the Division might have entered into these contracts without requesting official support. It had no other way of getting from the Services an expression of the relative urgency of gunnery training problems, however, and at this time that seemed the essence of the matter.

[4]The Prince Rupert's drop is made by dropping molten glass into cold water. Under these circumstances the glass forms a gourd-shaped drop with a long, curved, and tapered neck. The exterior surface of the drop is under a mechanical stress different from that in the interior. On fracture of the neck mechanical failure occurs throughout the entire drop and because of the net compressive strain that is relieved by the failure, the particles of glass fly apart, thus simulating a miniature explosion.

[5]Assistant Professor of Physical Chemistry.

[6]Vice-Chairman of NDRC.

[7]Present: Burchard, Curtis, Fairchild, Bleakney, Gross, Robertson, and Taub.

Moreover, this project would clearly require extensive use of the sort of facilities available to the NDRC only through Service support. NDRC had no airplanes of its own. Also, the project straddled two major Army departments, Ordnance and Air Forces. As the facts then appeared, the objection of either could reduce any research accomplishments to wasted effort. Finally, the division had to balance this project against other demands for manpower and money. Selective Service rules permitted the deferment of young men only when they were engaged on officially supported projects, and official support was not readily available for long-shot research. Certainly, had these barriers not existed, the subsequent history would have been shorter.

By September a conference was mounted in the Pentagon.[8] The subject was thoroughly surveyed. Everyone agreed the objective was desirable. The elements of a possible compromise solution were recognized, some armor on the plane, some scaling of bullet and plane speed, some modification of sight, some method of recognition. Ordnance raised a few difficulties which it did not press at this time: the bullet might break and jam in the gun, the ballistics might not closely enough match the Service projectile; the armor might be too heavy for the plane to fly; NDRC help was not needed; the Ordnance could develop the bullet itself if the Air Force would make the request specific. All these themes were touched upon but lightly. Agreement was that Air Force would request Ordnance to set up a project for bullet and gun and would indicate an approximate limit for the armor the target plane could carry. Ordnance would process this request through the Ordnance Committee, set up a project, and ask help from NDRC if needed. Burchard returned from this meeting to note in his journal that he was optimistic. He was actually inexperienced.

Things went on as before for a while. Fairchild worked for the Air Force proposal; Duke and Bakelite continued to work for free; Princeton co-operated. But then came trouble. When the AAF sent Ordnance the project request, it included only a requirement for bullet development, no mention of the rest of the program. Ordnance therefore would not give it official status. The Air Force officer on the Ordnance Committee was reported as being opposed to the project. Ordnance interpreted this as dissension within AAF and insisted that this be straightened out first. Meanwhile,

[8]September 1, 1942—"To discuss a Means of Simulating Combat Conditions in Flexible Gunnery Training," Lieutenant Colonel Domonoske OD presiding. Colonel Domonoske was then Ordnance liaison officer for NDRC, and co-operative. For the AAF, Colonel E. A. Lynn, Captain Fairchild and Captain E. B. Harwood; for Ordnance, Colonels S. B. Ritchie and Rene Studler and Lieutenant H. A. Ellison; for WDLO, Colonel W. H. Williams; for the USMC, Lieutenant Colonel I. L. Kimes; for Navy BuOrd, Lieutenant Commander G. B. H. Stallings and Lieutenant L. G. Pooler; for Navy BuAer, Lieutenant R. H. Price; for NDRC, Burchard, Robertson, Taub, Kelly, Curtis, Gross.

Burchard was constantly telephoning Colonel Studler as to the project status and getting answers such as the above. Perhaps after all, the gunnery problem was not critical. NDRC had no way of judging.

It was necessary to instruct Princeton to discontinue work on the project. Nevertheless, it was determined to set forth this conclusion and its reasons for general circulation in the Division Summaries,[9] and to bring out a formal NDRC report on the considerable progress already made.[10] This might stir official action. Meanwhile, informal co-operation could and did continue. Added to the contributions made by Princeton was the concept of scaling or reduction of range with concomitant scaling of gunners' sights, the results of computation by Robertson and Taub based on experiments by Curtis. This permitted lower muzzle velocities for the trainer bullet.

At this stage, however, the project was saved primarily by the patriotism and tenacity of Duke and Bakelite.[11] Bakelite continued to make bullets with metal fillings of various types, Duke to experiment with them, now assisted further by the voluntary aid of Dr. Katharine Jeffers.[12] Moreover, Gross and Fairchild continued to seek official support. On December 1, 1942, Gross wrote to the Director of Military Requirements, AAF, setting forth the situation as he saw it, expressing his faith in the project and urging that the Air Force clarify its position. To this, within three days, Gross received a courteous reply. This letter is worth quoting in part:

This office did initiate a project through the Ordnance Department which had for its object the development of a type of projectile which could be fired from standard aircraft machine guns against target airplanes for use in aircraft gunnery training. It was necessary that the target airplane be a normal airplane unarmored.[13] Although the project did not clearly appear to be possible of successful completion, nevertheless the value of such a projectile was so great, the interest of Captain Fairchild was so intense and the progress at that time had been so far above expectations, this office did believe that further study and investigation were warranted. The Ordnance Department basing their opinion on expe-

[9]Division 2 Project Summaries as of February 1, 1943, contained the statement: "During a considerable period the Princeton Station and Duke University conducted experiments which would be useful for a formal project, under the impression that the project would ultimately be established. Many conferences were held with appropriate Service representatives. Since a solution of the problem depends upon a joint attack on the plane armor, the gun used and the projectile required, the whole problem was obviously beyond the resources or the purpose of the Division. In view of the fact that appropriate Service interest has seemed not to be forthcoming, the project has been discontinued at the station in favor of projects that evoke more interest."

[10]NDRC Report No. A-210 (OSRD-1788) "Deformable Projectiles for Flexible Gunnery Training," by the Ballistic Research Groups, Princeton and Duke Universities, September 1943.

[11]A. J. Weith, Director of Research.

[12]Of the Department of Zoology.

[13]Author's note . . . why the target plane had to be unarmored does not appear in any NDRC record. In the long run it, of course, was armored.

rience and background in that department held little hope for its success and were not particularly enthusiastic about continuing the investigation. Only recently the Ordnance Department indicated that a target airplane would require armor in thickness from 3/16 to ¼ inch and possibly a little thicker. Such a requirement, of course, definitely precluded any further consideration of the project, the Ordnance Department being so advised.

The letter concluded with regret and expressed the hope that if the conclusions as to armor were incorrect the project might be revived. Dr. Gross replied on December 15, pointing out that, on the results to date, armor thicknesses above ⅛-inch steel would probably not be necessary. This elicited the following interesting response from the appropriate Assistant Chief of Staff (AAF), stating that the second letter had been referred to Experimental Engineering Section Material Command for reply. The reply is worthy of duplication in full:

A thorough study has been made by the Ordnance Department and by this office of the project directed toward the development of a special trainer bullet with certain desirable characteristics. It has been concluded that this problem *does not offer sufficient promise of early solution to justify setting it up and carrying it on as an official project at this time.*[14]

Your statement that, "at no time have we contemplated the possibility of using armor any thicker than the maximum of ⅛ inch at the outside," has been noted. The Director of Military Requirements had directed that the project be given no further consideration in view of the fact that ballistic experts have stated that the target airplane could not be used without some additional protection. His view was that, unless a normal unarmored airplane could be used, the project does not warrant further consideration.[15]

Your further statement, "these results at least indicate that projectiles made of different materials than the conventional ones can be fired at high velocities with much less penetrating power, and further that armor thickness above ⅛ inch is probably not necessary for protection against such projectiles," has also been noted. It is well known[16] that very light projectiles can be fired through the bore of the service rifle at high velocities and in many cases such bullets will give slight penetration. The files of the Ordnance Department include years of work along the lines of bullet development. There is certainly a question whether a bullet not conforming in weight, balance and profile to the service bullet would compare favorably as to trajectory with the service ammunition. If the bullet should happen to tumble in flight and strike other than point-on, the penetration would be expected to be less. *However, the trajectory of a tumbling bullet would* be very poor from the standpoint of target practice.[17]

[14]Author's note . . . Italicizing is mine. In view of the fact that successful firing trials in the air had been completed within fifteen months from the date of this letter after months of slow motion due to nonofficial support this was a remarkably bad guess.

[15]Author's note . . . See footnote 13. Echo answers "Why?"

[16]Author's note . . . A standard response when in technical difficulty.

[17]Author's note . . . The italicizing is mine. Why the assumption that the bullet would tumble is not clear. It was, of course, convenient.

At this point, temporarily Dr. Gross gave up seeking a formal project. He cannot be blamed.

With such help as they could muster Fairchild and Gross persisted. Fairchild did engage support from his own commanding officer, Colonel William Kennedy who, despite the awesome array of official opinion, continued to issue orders which permitted Fairchild to travel east approximately monthly to visit Duke, Bakelite, Princeton and the Corning Glass Works. The glass bullet work was discontinued in early 1943 in view of the greater promise of the plastic-metal combination. By late spring of 1943, the first mold for making molded bakelite blanks was ordered from funds provided by the Flying Training Command through Fairchild. This greatly expedited the production which had previously been by hand. In the summer of 1943, the command assigned Lieutenant Paul Greig, Lieutenant Homer Henderson and Corporal Jenneman to assist Fairchild. Greig, an Ordnance officer, remained with the project until other assignment in 1944, Jenneman until he returned to Laredo in early 1945. Henderson, who rose to Major before his discharge in September 1945, ultimately succeeded Fairchild in the project and was a key man in all the later stages.

By late summer 1943, it appeared clear that lead-filled bakelite bullets of high density could be fabricated with sufficient frangibility. At the same time progress had been made in the selection of powders for the completed round with the assistance of Lawson and others of the du Pont organization; considerable work had been done on the testing of armor for possible use on the target airplane.

Meanwhile, Division 2 had been waiting for evidence from the combat areas of the need for better gunnery in order to return to the attack. This evidence was finally forthcoming in a report by Air Force General Ent as a result of a survey tour in the Mediterranean Theater. Armed with this report, Burchard addressed a letter to Carroll Wilson, Special Assistant to Dr. Bush, in October 1943, pointing out the emphasis on inadequate training of flexible gunners in theater intelligence reports and asking for a reopening of the frangible bullet question. This resulted in a conference on November 9, 1943, presided over by Dr. J. B. Conant, Chairman, NDRC.[18] It was generally agreed that live training must be accomplished and that the frangible bullet looked hopeful. A special committee was appointed to report further and to recommend a specific program for eliciting high Service support, foreseeing and answering all possible technical objections.[19] This committee met November 19, 1943, at the call of Dr. C. P.

[18]Present: Dr. Conant, presiding; Carroll Wilson; W. S. Hunter, M. S. Viteles, C. W. Bray of Psychology Panel; Churchill Eisenhart of Mathematics Panel; S. H. Caldwell, Chief, Section 7.2, Division 7; W. S. Gorton, Section 17.3; Bleakney, Curtis and Kelly, Division 2. This was an NDRC meeting with no Service representation and typical of the way NDRC operated under such circumstances.

[19]Made up of Eisenhart, Viteles, Caldwell, Gorton, Curtis, Bleakney, and Gross.

Haskins, Executive Assistant to the Chairman, NDRC.[20] Curtis, Gross and Viteles were authorized to prepare a report for submission to Drs. Conant and Bush. This report was tendered on December 1, 1943. The Director engaged in conversations with the Air Force. At the same time, other pressures were developing from within the Air Force. Also, tests on the armor proposed for the target plane had been conducted at Wright Field and were very promising. The concatenation of the three events led to a request from the Air Corps Training Command for formal initiation of an NDRC project late in December 1943. Before he would support this request, Brigadier General B. W. Chidlaw, now Air Force Liaison Officer with NDRC, asked for damage trials of an armored wing section of an A-20 with frangible bullets. These trials were satisfactorily completed January 7, 1944. On January 17 Brigadier General R. W. Harper, Assistant Chief of Staff for Air Training, formally requested initiation of an NDRC project. The project was accepted, and late in February Gross held conferences with key personnel in Division 2 (Burchard, Bleakney, Curtis, Taub and Kelly), resulting finally in the preparation of a contract with Duke University so that after March 1, 1944, it would no longer have to carry the burden with its own funds. This permitted a considerable enlargement of personnel at Duke and the writing of a subcontract with Bakelite Corporation in April 1944. From this point on, the administrative hurdles were slight and the lesson of this story is ended.

Such an account has perforce made light of the technical difficulties. For example:

a. A good frangible bullet with good ballistics had to be worked out.
b. A plan for vectorial scaling of bomber's and fighter's velocities, the bullet's muzzle velocity and the ring sight's size had to be made.
c. A target plane had to be armored and to be able to fly.
d. A hit indicator system had to be designed.
e. A device to adapt the standard .30-caliber machine gun for use with frangible bullets had to be provided.[21]

Under the Bakelite subcontract directed by Weith,[22] experimental production of bullets was begun. The Duke work continued under Gross and Hobbs with an augmented staff.[23]

During the winter and spring of 1943–44, the standard .30-caliber machine

[20]Present: Haskins presiding; Davidson and Teeter from Chairman's Office, NDRC; J. B. Russell, Section 7.2; Taub and Kelly, Division 2.

[21]Mostly built by Fred Kuhn, mechanician of the Department of Chemistry, Duke University.

[22]Assisted by Dr. C. E. Staff in charge of experimental work, V. E. Meharg, A. P. Mazzucchelli, L. E. Welch, W. A. Miller and R. E. Nicholson.

[23]Including Carl Deal, A. J. Weith, Jr., Harold A. Scheraga, and Drs. J. H. Saylor, D. G. Hill, F. London and K. E. Zener.

gun had been modified at Duke so that it would recoil properly with the lighter powder charge and mass of the frangible bullet. The modification made use of a piston booster at the muzzle to add to the barrel's momentum recoil. Two such guns were then installed in the upper Martin turret of a modified B-17 (YB-40) and tested on the ground at the target butts, using 1500 rounds of ammunition turned by hand from Bakelite molded blanks and hand-loaded at Duke. Two returned A-20 pilots with extensive combat experience in the Pacific, Captains Charles T. Everett and J. B. Roan, had been ordered to Buckingham Field as pilots for the A-20 target plane, "Alclad Nag," their orders reading that they were to try out a new kind of ammunition on a volunteer basis. Surprised as they were to learn they were to be shot at with live ammunition, they expressed willingness to take the plane up and be shot at after watching Gross, Fairchild and Joseph Evans of Wright Field each take his turn in the cockpit while frangible rounds were fired at a range of twenty-five yards. The assignment of the pilots was most fortunate for they were especially skillful and flying the "Alclad Nag" was no simple feat.

The first air-to-air firing trials were conducted successfully on May 29, 1944, with Captain Everett piloting the target plane. After several dry runs, Sergeant Karp in the upper Martin turret fired, while Sergeant Oldham called the range from the upper Sperry turret. Hits were made and they were registered by the flashing light in the target plane's nose. Further trials that day and the next were equally satisfactory, but in the afternoon of the second day, catastrophe threatened. One engine of the "Alclad Nag" went out because of mechanical failure and the heavily armored plane began to fall rapidly. Captain Everett refused to jump and by skillful and courageous piloting brought the plane to an emergency landing, about twenty miles from Buckingham at Page Field. This was an important contribution, for the loss of the only available experimental plane at this moment might have been fatal to the development. Further trials were successful on June 13, and the frangible bullet was over the hump.

The rest of the history of this development is more nearly routine. Gross and Fairchild collaborated with Major General Harper in working out plans for a training program which was rapidly put into effect. Improvements of the hit indicator system were made with the help of Curtis, Lampson and Lieutenant Henderson. Conferences were held to settle on the final target plane, which was to be a modified P-63. This brought the Bell Aircraft Company into the development, and from there on their energy also aided the rapid development. On July 18, 1944, all aspects of the production procurement program were specified. The first armored RP-63 was ready to fly by September 1, 1944, a remarkable achievement by Bell Aircraft. These trials, too, were successful. About this time, studies were made to improve the pick-up system for registering hits, and three different good ones were

worked out by Duke, Sperry Corporation, and Bell Aircraft. Modification of sights to fit the ballistics of the gun began with a conference at Aberdeen in September 1944. Aberdeen agreed to determine the ballistics of the frangible bullet and work out Siacci functions and firing tables, for use by the sight manufacturers in modifying their sight. This was completed by early fall but little work was done towards modifying sights until early 1945. In the meantime, sights had been modified at Duke by Captain Henderson and were used successfully by student gunner groups. In the fall of 1944, Colonel E. M. Day was appointed commandant of the Laredo Army Air Field and quickly introduced the method into training. Some further real difficulties were encountered with the B-29 turrets, but these were solved in turn as was the armoring of the target airplane so that it could attack head-on as well as from the rear quarter. In March 1945 the Air Forces gave a public demonstration, which was Major Fairchild's last important connection with the project. Illness and assignment to other duties kept him out of the project until his discharge from the army two months later. He received a well-earned Legion of Merit for his steadfastness of purpose.

Active work continued on all phases of the project through June and July, and in August until V–J Day. It is of interest to summarize briefly the over-all status of the frangible bullet technique as it existed just prior to V–J Day. At this time about 300 of the original RP-63-C type airplanes were in use in seven gunnery schools in this country and some 11,000 bomber missions had been flown by student gunners in which 13,000,000 rounds of frangible bullet ammunition had been fired. About 450 improved-type RP-63-G airplanes had been ordered for delivery beginning in September 1945. By August 1945 the scheduled production of frangible bullet rounds was to have been about 45,000,000 per month. Just before V–J Day, the statement was made that "all firing from the air in the gunnery training program of the Training Command will be with frangible bullets."

Last but not least, in all of the frangible bullet operations, no injuries, fatal or otherwise, were suffered by any target airplane pilot.

This project has been described in more detail than most of those of Division 2 for three reasons. First, it is one of the most interesting, illustrating perfectly from what unlikely origins a device of the greatest importance may spring. Secondly, it illustrates clearly how many groups had to co-operate to make a war research go: Duke, Princeton, Bakelite, Corning, Sperry, Bell, Wright Field, Aberdeen Proving Ground, and the Air Force Training Command, to name only the principals. Finally, and most important, it illustrates all too clearly how badly organized in many ways we were for research in war. The project might, but for the tenacity of two men, have been another case of too little and too late. It was contrary to much traditional Ordnance background and was seriously opposed (and on the

record) by Ordnance. Selective Service policies at the time made Ordnance objections an effective barrier to the introduction of NDRC in a formal way into the project at the most critical time. The Chief of Division 2 did not have enough courage, interest, or military experience at that time to flaunt the procedures, wangle around the Selective Service regulations, and carry on anyway. No person in his organization seriously attempted to discourage him from his decision. The Director of OSRD did not have enough power to provide a division with airplanes and other necessary gear to carry on despite all opposition. Thus, valuable months were frittered away; perhaps a year was lost, or at least used less effectively than it might have been. In the last analysis, one or two willful men in the Ordnance Department nearly stopped the development altogether; one or two willful men outside the Ordnance Department and outside the NDRC kept it alive. This is too slender a thread on which to hang the fate of a research. It suggests a more independent procedure for the handling of possible future innovations of military value.

# APPENDIX

## *LIST OF DIVISION 2, NDRC, CONTRACTS*

| *Contractor* | *Director* | *Subject* | *OEMsr- Number* (except as noted)* | *Effective Date*** | *Supple- ments§* |
|---|---|---|---|---|---|
| Cal. Tech. | von Karman | Wind Tunnel | NDCrc-36 | 11/18/40 | |
| Cal. Tech. | Clark | Rapid Strain | 348 | 3/1/42 | (3) |
| Cal. Tech. | Millikan | Muzzle Blast | 1351 | 5/25/44 | |
| Carnegie | Seitz | Rapid Strain | 825 | 12/1/42 | (3) |
| Carnegie | Pugh | Shaped Charges | 950 | 4/1/43 | (8) |
| Cornell | Kirkwood | Explosions | 121 | (in Div. 8) | (6) |
| Cornell | Kirkwood | Rapid Strain | 751 | 10/2/42** | |
| Duke | Gross | Trainer Bullet | 1248 | 3/1/44 | (1) |
| Franklin | Allen | Muzzle Brake | 1398 | 9/1/44 | (3) |
| Genl. Elec. | Robinson | Muzzle Blast | 1343 | 4/24/44 | (1) |
| G. Wash. U. | (Not really started because war ended — was for AN-23) | | | | |
| Harvard | Bridgman | High Pressure | 201 | 10/1/41 | |
| Herbach | Bunkin | Mobile Laboratory | 424 | 4/3/42** | |
| Illinois | Richart | Impact Tests | 318 | 2/1/42 | |
| Illinois | Newmark | Damage Analysis | 1476 | 4/27/45 | |
| Little, A. D. | King | Damage Analysis | 1498 | 9/1/45 | |
| M.I.T. | Wilbur | Impact Theory | 468 | 3/1/42 | (2) |
| M.I.T. | de Forest | Rapid Strain | 641 | 7/1/42 | |
| Penna. | Seitz | Rapid Strain | 132 | 9/1/41 | |
| Penna. | Seitz | Rapid Strain | 336 | (cont'd same) | |
| Polaroid | Land | Plastic Armor | 213 | 10/16/41 | |
| Princeton | Bleakney | (general) | NDCrc-34 | 11/1/40 | (1) |
| Princeton | Bleakney | (general) | 260 | (cont'd same) | (10) |
| Princeton | Bleakney | Mobile Laboratory | 675 | 8/7/42** | |
| Stanolind | Silverman | Gauges | 596 | (in Div. 8) | |
| Westinghouse | Nadai | Rapid Strain | 891 | 11/1/42 | |
| Woods Hole | Cross | (general) | 569 | (in Div. 8) | |

* Unless otherwise noted, OEMsr- is understood to accompany number.

** When marked, this is proposal date; otherwise date contract effective.

§ Number in parentheses is number of supplements, roughly, when known. These figures are far from complete, and are not accurate. They are included only as an indication of the activity along some lines.

PART THREE

# Hypervelocity Guns and Control of Gun Erosion

*The History of Division 1, NDRC*

# *ACKNOWLEDGMENT TO PART THREE*

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ORVILLE H. KNEEN

Washington, D.C.
May, 1946

## CHAPTER XXXI

# *THE QUEST FOR A SUPER-GUN*

"The history of warfare is the history of decisive changes in the destinies of nations made by the invention of new weapons."[1]

IN THE PAST two centuries the gun has become the nucleus around which armies, fleets and aircraft have revolved. Until recently it was a long-lived weapon, partly because, whether large or small, it was loaded and fired by hand. The barrel lasted about as long as the rest of the gun.

The trend of modern warfare has completely changed this. For many years gunners have been calling for higher and higher muzzle velocities, not only to improve the chances of making a hit, but also to increase the effective range of their projectiles. Velocities could readily be increased by firing larger powder charges, but this rapidly increased the erosion of the gun-bore as well. The demand for greater striking power in land artillery and naval guns had to be met by increasing the weight of powder fired, at the cost of reducing the gun life. Since World War I, higher velocities in all calibers of guns have made even the improved gun steels unsatisfactory *at the gun-bore surface.*

Smokeless powder, widely used since its invention in the 1880's, proved much more erosive than black powder had been. Rapid-fire guns, in small sizes and more recently in medium-sized guns, further added to the accumulating problems of the gun designer. Barrels began to wear out at an almost prohibitive rate as the rapidity of automatic fire was increased and as it became necessary to fire longer bursts. In World War II the need for better guns soon became apparent. Antiaircraft guns were fired until their smoking barrels blistered the gunners' hands. As fighters and even bombers swooped down upon land targets, aerial gunners fired longer and longer bursts in strafing or in aerial attacks against each other. Red-hot (and ruined) barrels were the order of the day; aircraft often came back from a single sortie with one or more machine guns "burned out." Sometimes lives and aircraft were lost because machine guns suddenly lost their accuracy. The steel had become overheated, erosion and wear had been greatly accelerated, and the rifling had been wiped off. The bullets thereafter were not properly rotated; tumbling and dispersing, they failed to reach the target while the enemy continued to fire with precision.

[1]From an editorial in the *New York Times.*

The National Defense Research Committee began casting a critical eye on the inadequate gun barrel sixteen months after the fall of Poland. As early as December, 1940, the subject of gun erosion was called to the attention of Professor A. E. White of NDRC, during a visit to the Frankford Arsenal; at Watertown Arsenal, White also discussed erosion as a problem of military importance with Lieutenant Colonel (now Colonel) S. B. Ritchie.

Early in 1941 Dr. James Bryant Conant, then a member and later Chairman of NDRC, returned from his mission to Britain with the conviction that gun erosion was "one of the outstanding problems from a defense standpoint" because of its bearing on hypervelocity. Before Conant returned, Dr. Richard C. Tolman, visualizing the possibilities, had begun conferences with the Army and Navy. He found them unwilling to request NDRC to enter the hypervelocity field.[2]

About this time a review of erosion was made by Colonel G. F. Jenks at the request of Brigadier General R. H. Somers.[3] He gathered many of the prevailing ideas on the causes of erosion, and listed various factors requiring research. There had been no organized programs in which modern research methods had been applied to improve gun design. Although an investigation of some phases of gun erosion had just been started at Battelle Memorial Institute (Columbus, Ohio), under contract with Watertown Arsenal, Colonel Jenks pointed out that it was on too small a scale to be expected to give a real understanding of the mechanism of erosion or a practical solution of the problem. Furthermore such research was considered to be outside the sphere of interest of the metallurgical industry.

In spite of Colonel Jenks's earnest recommendation that the subject be thoroughly and adequately prosecuted, using several laboratories with well-trained personnel, both Services early in 1941 concluded not to make a formal request for an investigation of gun erosion by NDRC; although both expressed willingness to co-operate to the extent of furnishing information and materiel if NDRC independently set up such a project.

Nonetheless NDRC, having decided that investigation of gun erosion and hypervelocity was needed, created a new section in Division A, designated as Section A.[4] On June 13, 1941, Dr. L. H. Adams accepted its chairmanship. The assignment given to Section A–A was named "Erosion and Special Factors in Gun Design," with the term "Special Factors" used to camouflage the real and main objective of hypervelocity.

Adams was Director of the Geophysical Laboratory of the Carnegie

[2]Hypervelocity has been considered by Division 1 as a muzzle velocity of 3500 feet per second or higher. This is some 500 feet per second greater than that of the most powerful guns previously standardized by our Army and Navy.

[3]Then Chief of Technical Staff of the Ordnance Department and War Department Liaison Officer with NDRC.

[4]"A" for Adams, its Chairman. It was commonly termed "Section A–A."

Institution of Washington. Dr. Bush was President of this Institution, the Trustees of which had offered to make available to the Government, without charge, the services of its staff members and the facilities of its various departments to aid in solving defense problems.

The gun-erosion problem impressed Adams as one that the Geophysical Laboratory was much better able to handle than an outsider might suspect. Its staff of some twenty investigators included chemists, physicists, and geologists. The experiments they had been conducting in an effort to learn the secrets of mineral formation had involved the use of high temperatures and high pressures, two of the principal characteristics of the powder gases in a gun. Above all, they were well schooled in the methods of scientific research.

The summer and fall of 1941 were spent in planning and preliminaries. Following the pattern that NDRC had already found effective in getting emergency research under way, a Government contract for $10,000 was arranged with the Carnegie Institution, effective July 15, 1941, so that the staff of the Geophysical Laboratory might be augmented and special equipment purchased.

At about the same time several of the regular staff members of the Laboratory were given Government appointments as volunteer "Consultants" to Section A–A, and most of the remainder of them were introduced into the program in this way later that year. This arrangement facilitated their entry into Army and Navy circles for the purpose of learning the background of the problem.

Four of these staff members remained at the Laboratory only a few months after the erosion program had been started, and then were called to other war service activities. During that early planning period, however, Adams found their advice very valuable. Dr. F. E. Wright suggested a number of original approaches to the study of the mechanism of erosion that later were followed successfully by other members of the staff. Dr. C. S. Piggot was largely responsible for the design of the Laboratory's first firing range and ordnance testing building. Dr. R. W. Goranson had had a prior interest in gun erosion and in a memorandum to Bush in May 1941 had suggested an experiment for determining the effect of stress on this phenomenon, an experiment that he helped start. Dr. R. E. Gibson was concerned briefly with plans for the study of powder gas temperatures; and even after he transferred his attention to the work of Section H of Division A he continued to keep in touch with the erosion program and served to unify certain ballistic phases of it with similar ones in the rocket program.

Both the Army and the Navy helped Section A–A in its search for information about gun erosion. Dr. J. S. Burlew, another staff member of Geophysical Laboratory, serving as a Section Consultant, worked full time for five months in the Army Ordnance Department, combing its confiden-

tial files for clues that might lead to a solution of how to mitigate gun erosion.[5] This survey emphasized that no solution of the hypervelocity problem was to be expected until the fundamental causes of erosion were known.

The first experimental work on the new problem at the Geophysical Laboratory was started by Goranson, Dr. Eugene Posnjak and H. S. Roberts. Posnjak began to study the effect of nitrogen on steel at high temperatures and Roberts and Goranson separately began the development of pressure gauges suitable for measuring explosion pressures. At the same time Dr. George Tunell was accumulating information concerning the latest techniques, such as X-ray and electron diffraction photography, for studying the nature of thin films on the surface of steel. Dr. J. W. Greig, beginning in December 1941, helped organize the Section program and later supervised some of its contracts in addition to heading a group at the Laboratory working on the improvement of machine-gun barrels.

After the Laboratory's experimental program had been completely laid out, as outlined in Chapter XXXII, Dr. E. G. Zies organized a group for the investigation of the chemical aspects of gun erosion, and served as a special adviser to a number of the division's other contractors with respect to this phase of the program. Dr. F. C. Kracek introduced a new technique into experimental interior ballistics by developing a spectrometric method of measuring the temperature of the burning powder in a gun barrel. O. H. Loeffler and Dr. F. E. Ingerson developed experimental methods for studying erosion in the laboratory. Dr. E. F. Osborn, concerned principally with the development of chromium plate and other resistant materials, represented the section on a number of occasions when other contractors were being induced to take up particular parts of this phase of the program.

This listing of geologists who so successfully became ordnance specialists would not be complete without mention of Dr. H. E. Merwin. Although he did not happen to have been appointed a Consultant, he was called upon frequently by the Section Chairman for advice after he had applied his previous skill in microscopy to the description of the pattern of erosion in gun bores.

Visits were made to Aberdeen and Dahlgren Proving Grounds and to Watertown and Watervliet Arsenals as part of the education of some of these civilian scientists, few of whom knew anything about guns when they started their project. By the end of the summer the Section was beginning to develop a clearer picture of the interior of the gun barrel. Also, some possible answers were being formulated for the hitherto baffling question: Why do gun barrels wear out?

The better the answer to this erosion problem the longer the gun would

[5]As Burlew expressed it, "obtaining information which, in many cases, was partially buried many years ago." See p. 362.

last. More important still, if erosion were decreased the gun could be made more powerful and, it was hoped, still have a useful length of life. This involved — for the first time in the history of ordnance — a thorough investigation into the causes and prevention of erosion, with the objective of laying the groundwork for higher muzzle velocities. The search for the fundamental knowledge of gun erosion thus became in effect, a long-range quest for a super-gun.

Any sportsman who has used a high-powered rifle knows how erosion proceeds. But no one could tell the civilian "gun scientists" how it was caused. They were aware that the sharp-edged rifling of a new gun (shallow spiraling grooves down the bore that impart high spin to the projectile) gradually becomes worn, especially where it starts just ahead of the chamber. When this section of rifling is obliterated, the projectiles are no longer given the proper rotation and fail to fly true to the course for which they are aimed.

When that point is reached, the barrel usually must be discarded, perhaps even the entire gun. Two major factors determine this end of accuracy-life for a given gun: The rapidity with which it is fired, and the size of the powder charge. Accuracy-life varies greatly; the firing that a 16-inch gun will withstand is less than 200 rounds, medium-sized guns some 700 rounds, while the machine-gun barrel of 1941 would stand up for several thousand rounds, *provided* that the firing was done in short bursts, and the barrel was cooled off between bursts. But the machine gun of that day would not stand a *continuous* burst of more than 170 rounds — less than ten seconds of firing.

The size of the powder charge was of special interest to NDRC at the beginning of its erosion project. The larger the powder charge, the higher the muzzle velocity of the gun, that is, the faster the projectile will be shot forth. A higher-velocity projectile gets to its target more quickly. Doubling the initial velocity of an antiaircraft projectile, it can be calculated, will increase very greatly the chance of hitting an airplane from the ground. The depth to which a projectile will penetrate armor plate will in general increase with the striking velocity.[6] A velocity as high as 5000 feet per second might be desirable for the most effective penetration of tank armor, which had become increasingly heavy. Furthermore, the greater the impact velocity of the projectile fired from an airplane, the more destruction it is likely to cause when it hits another plane.

The demand for higher muzzle velocities had been met over the years by repeated improvements in ballistics and in gun design, but was never fully satisfied; for as soon as a higher level of performance was reached, there came an urge from the forces in the field to have it increased still further. But there was always a limit, the limit imposed by erosion.

[6]In this connection, however, see Division 2 history, p. 276.

The gun designer, who had come to accept erosion as being as inevitable as old age in mankind, was forced to adopt a compromise between high velocity and long life. He could not have them both in one gun. From experience he knew that if a projectile heavy enough to be effective was fired with a muzzle velocity exceeding about 3,000 feet per second, the life of the gun would be distressingly short. But even that speed — over 2,000 miles an hour — though greater than that of the average projectile, is not enough for the purposes of modern war.

The guns developed by our Army and Navy by the time we entered the war were superior in many ways to those used in World War I. The muzzle velocities of many of them had been increased; but still, with one exception, they were all less than 3,000 feet per second, in order that their lives might be reasonably long. A few officers had advocated higher muzzle velocities by using heavier powder charges, feeling that the advantages of increased velocity outweighed the penalty of shorter gun life. The Germans in North Africa gained successes in the summer of 1942 by stepping up the powder charges and producing higher velocities, at the cost of gun-life.

Section A–A, in the interest of speed, was ready to have its investigators follow every possible clue that held hope of solving the basic erosion problem, or of aiding its solution in any degree. Within two months of his assignment to the job, Adams had planned extensive and difficult investigations on the pressures, temperatures and composition of gases produced by firing; types of steels used in guns; rotating bands of projectiles. A series of studies was projected, chemical, physical and metallurgical, to determine the actual mechanism of erosion.

For these studies the latest and most modern techniques were to be applied, such as the use of radioactive tracers to determine the types of chemical reactions. Some of these techniques had not previously been adapted to use for gun-bore studies, and their application called for ingenuity and resourcefulness.

Less than two months after he had assumed the role of Chairman of the new section, Adams outlined in a letter[7] to Tolman the current status of the erosion problems and indicated the broad lines along which it was considered possible to solve them. In addition to studies which were admittedly long-range and perhaps would be unproductive of tangible information for some time, there were to be no restrictions on trying any promising item for immediate application, or "playing the hunches" if they seemed to have reasonable merit.

In the middle of October 1941, the Section had the opportunity of presenting its general program and plans at a conference comprising some thirty ordnance specialists of the Armed Services. As described in Chapter XXXII, a day-long discussion of erosion and hypervelocity brought out the many ex-

[7]This letter is quoted in Appendix 3. See p. 453.

planations and opinions that existed as to the possible causes and cures for erosion, and means for the practical attainment of higher velocities. Although some long-range studies were approved, the Services remained generally pessimistic as to NDRC's chances of achieving practical results "for use in this war."

The NDRC men, however, were more than ever confident that coordinated investigations by some of the country's leaders in research could penetrate the mysteries of the gun barrel. If so, better guns could be built in time to win a war we had not yet entered, but were now clearly destined to join.

By the end of October 1941 reconnaissance of selected parts of the field was well advanced. The Geophysical Laboratory had made preliminary experiments. Adams reported that a gun yielding a velocity of perhaps 5000 feet per second was not impossible to design, but that gun steel would erode far too rapidly. Incomplete knowledge as to the complex factors causing erosion, and lack of agreement as to their relative importance, had resulted in several distinct theories of erosion. He pointed out that: "An approach to a real understanding of the nature of erosion obviously requires quantitative investigation along several lines."

Some practical ordnance men recommended early that NDRC's long-range studies (aimed, they then felt, at future uses rather than for the war that was imminent) include development of gun steels with increased resistance to erosion. Section A–A might have responded by preparing to analyze, test, and if possible improve any type of steel ever made, or any ferrous (high iron-content) composition that resourceful metallurgists could devise.

But the insistent question kept arising: Why use steel? The gun designer seemed to have all the answers to that question: gun steel was the only material with the strength and elasticity to resist indefinitely the explosive and impact forces of firing, and at the same time the availability and workability required for a gun barrel material. Gun steel was ideal, it might be conceded, save for one defect — thin layers on the bore surface were continually being carried away under the successive thermal and chemical attacks of the white-hot gases that follow the projectiles down the gun bore.

That defect was fatal to the gun barrel. It was Section A–A's job, then, either to find a gun steel or some other material that would not give way under powder-gas attack, or to develop some method of protecting the steel. So attractive were the possibilities of devising protection for the gun-bore surface that groups subsequently were assigned to the study of platings and coatings, while separate investigations were begun on the subject of liners.

The search for a truly erosion-resistant material became an important part of the Division I program, engaging the attention of nearly half its contractors, and was destined for success, as described in Chapter XXXIV.

Some observers suggested that perhaps the basic trouble in large guns was not so much powder-gas erosion as mechanical wear caused by the terrific grinding of the projectile's rotating band against the gun's rifling. One method of mitigating mechanical wear (possible adaptation of an old idea), was the use of a "pre-engraved" projectile, that is, one provided with teeth which are set to engage with the gun's rifling. Experiments at the Franklin Institute were to demonstrate that guns which have been chromium-plated suffer much less erosion on firing pre-engraved projectiles than when using standard projectiles.

It was also felt during these first weeks of planning that perhaps some practical devices or methods would show up or could be developed from older ideas. These might be short-cuts leading to useful ways of achieving the primary objective of hypervelocity, that is, projectile velocity of 3500 feet per second, or higher, without undue barrel erosion.

A promising idea, later perfected by the division, was the old "sabot projectile," first used in a crude way by the French nearly a century before. Its purpose was to permit firing projectiles in larger-caliber guns, the "sabot" or carrier filling the space between projectile and gun bore, and being dropped after leaving the muzzle. The lighter projectile acquired higher velocity from the same powder charge, and its smaller diameter enabled it to retain the velocity because of lower air resistance.

Division 1 subsequently demonstrated, through the work of the University of New Mexico, the Geophysical Laboratory, and the Remington Arms Company, that an existing high-velocity gun can be made even more powerful so far as armor penetration is concerned by firing from it at a still higher velocity a sabot projectile having a tungsten carbide[8] core. Although both the British and the Germans used such projectiles in the war, it was not until near the end that our Army Ordnance Department was convinced of the value of this weapon.

Still another partially developed device for stepping up the velocity of a projectile was the tapered-bore gun. A lightweight projectile having "skirts" is squeezed down to a smaller caliber as it travels through the tapered bore and thereby acquires better ballistic characteristics. This ingenious principle had been known for over half a century, and had been tried with limited success by the German Gerlich and other inventors. Division 1, applying engineering principles, was able through its contractor, Jones and Lamson Machine Company, to make it into an acceptable solution of the hypervelocity problem. The division's pioneer work with a 57/40-mm. tapered-bore gun firing skirted projectiles at a muzzle velocity of 4200 feet per second caused the Navy Bureau of Ordnance to continue the development of this type of weapon after the termination of the division's activities.

[8]Tungsten carbide is one of the hardest of substances, ranking close to the diamond. See p. 281.

On December 3, 1941, Section A–A held its first meeting, at the Geophysical Laboratory. Besides Adams as Chairman, the members of the section were then Dr. E. R. Weidlein, Director of Mellon Institute, Pittsburgh, and Dr. Lyman J. Briggs, Director of the National Bureau of Standards. These well-qualified leaders of research, assisted by Burlew and Greig as full-time Consultants of the section, proceeded as a board of directors to consider the entire program. Assignment of specific tasks began and the first steps were taken toward initiating experimental work on a war footing.

Thus, four days before war was declared, Section A–A was a going concern, with a reasonably clear idea as to its mission, its specific objectives, and the forces required. During the previous summer a section in Division B of NDRC, under Professor A. E. White, had become interested in gun erosion as one of various metallurgical problems on which it was working. Unnecessary duplication of effort was avoided by White's prompt and graceful action in discontinuing his section's gun-erosion studies.

When Pearl Harbor made reality out of conjecture, a separate office was set up to handle Section A–A affairs as a Government agency and coordinating office. For convenience, it was located at the Geophysical Laboratory, with Burlew and Greig assisting Adams in its operation. The day after Christmas of 1941 Adams submitted to Tolman a proposed budget for a period of 18 months beginning January 1, 1942. It called for $200,000 for the first six months and an additional $550,000 for the next year to cover four general fields of research and development, two in erosion and two in hypervelocity, including attention to problems in interior ballistics that related to both main subjects. The budget was promptly approved by the NDRC.

By February 12, 1942, the Geophysical Laboratory had cleared its decks. On that day a staff meeting was called to consider a detailed outline of projects to be carried on at the Laboratory; assignments of specific tasks were made accordingly. With the speed that war engenders, the Laboratory's peacetime equipment in experimental petrology, such as that used for the production and measurements of high pressures and temperatures, and the application of other experimental techniques to the study of minerals and rocks, was replaced by such war equipment as explosion chambers and gun tubes.

Thus for the first time in its nearly forty years of existence the Geophysical Laboratory, a department of the Carnegie Institution[9] that had emanated from one of the world's great planners of peace, was called on to lead in the basic design of death-dealing devices.

The program was now expanding in various directions. Techniques were

[9]Founded by Andrew Carnegie. During World War I the Geophysical Laboratory was engaged in developing optical glass for manufacturing, an exceedingly critical enterprise for that day, but of course a relatively peaceful pursuit.

being developed to detect the products of erosion and to simulate erosion in the laboratory. Beginning January 15, 1942, when a contract was made with Western Electric Company for electron-diffraction studies, contracts were awarded to some of the country's finest research institutions — academic, consulting, and commercial.

As new contractors were brought in, they sent their investigators to the Geophysical Laboratory to learn about guns. There they could examine the remains of guns that had given their lives to their country. Few of these investigators ever had seen the inside of a gun barrel. By midsummer fourteen contractors were at work.

During the spring the growing list of contractors was paralleled by expanding membership in the section. Dr. H. B. Allen, Director and Secretary of the Franklin Institute, Philadelphia, became a member March 20, and two months later was made Vice-Chairman. Edwin L. Rose, Director of Research for Jones and Lamson Machine Company, became a Consultant March 14 and a member June 4, 1942.

A long step forward was taken on March 10, 1942, when Section A–A received formal Army Ordnance recognition of its erosion and hypervelocity program by the assignment to it of project number "OD-52." In their negotiations with Ordnance Department representatives during the preceding six weeks Adams, Burlew, and Greig had been helped by the sympathetic attitude of Major General C. C. Williams, former Chief of Ordnance and then Research Liaison Officer for the Ordnance Department, and by the practical advice of S. Feltman, head of the Ballistics Section of the Ordnance Department, who subsequently was designated as the liaison chief for Project OD-52.

Shortly afterward the Navy Department set up a pair of projects to show its interest in the section's program: NO-23 on "gun erosion" and NO-26 on "hypervelocity." Lieutenant (later Commander) B. D. Mills of the Bureau of Ordnance was named liaison officer for the former project and assisted Captain F. F. Foster and his successors in maintaining liaison for the latter one as well.

After formal working relations had thus been established with the Army and Navy, the section had access to all available data pertaining to its program, regardless of military classification. Its contractors were able readily to procure scarce war materials and to obtain deferment for their key employees. Also both the Army Ordnance Department and the Navy Bureau of Ordnance supplied various types of guns, including worn-out ones for examination.

"Erosion made to order" gradually became a procedure of prime importance. Investigators seeking basic causative data could not obtain much help from records made by the firing of guns in service; the erosion-causing factors were too numerous, and as firings could not always be con-

trolled, the factors could not be isolated from each other. Metallographic examination of artificially eroded surfaces proved very valuable; many samples of steel and other materials were exposed to powder-gases under laboratory-controlled conditions, and the resulting products examined and analyzed.

The vent plug proved an instructive initiation into the difficulties of measuring erosion effectively, especially before making a material into a gun barrel or liner. The vent plug was a well-known device whereby exploded gases in a closed vessel were exhausted through a small opening (1/16- or ⅛-inch in diameter); weighing the eroded plug measured the extent of erosion.

Many tests were made with various vent plugs, in the hope that a technique could be devised for forecasting accurately the resistance of a given material. But the best that could be done was to gain an idea of the relative order of erosion resistance of a group of materials proposed for gun barrels or liners.

In another series of experiments useful indications of the relative erosiveness of different propellants were obtained by venting their gases through steel vent plugs.

From such investigations it became clear that bare steel, or any alloy high in iron content, was not the material for high resistance to erosion, particularly against the chemical attack of powder gases near the breech end, where erosion was most severe. While the active search for protective coatings went forward, an even more energetic campaign was underway to discover, or invent, an "ideal" erosion-resistant material with the requisite high melting point and hardness at high temperatures. Even if not strong enough for a gun barrel, such a material, it was believed, at least could be used as a long-lasting liner for the standard steel barrel.

Contractor after contractor came into the growing group, as new materials showed promise in laboratory tests and it became desirable to institute searches for improved forms of these materials. Vast numbers of samples had to be fabricated, physically tested, formed into liners, heat-treated and otherwise processed, tested and then studied by metallographic, chemical and other laboratory procedures. As new types of barrels and liners were evaluated, and accuracy life was extended, the number of firings increased greatly, until an amazing number of rounds were being fired daily by three contractors, the Crane Company, the Geophysical Laboratory, and the Franklin Institute. Unremitting labor uncovered the clues that eventually were to lead to the development of the several new alloys for gun-barrel use, that are described in Chapter XXXIV.

Constant need was felt for fundamental knowledge about what actually goes on in a gun barrel. For example, it became desirable to determine experimentally the friction of a projectile as it is propelled through the

barrel. Ballistic studies for this purpose were developed by Dr. H. L. Curtis of the National Bureau of Standards, whom Adams had appointed as a Consultant to Section A–A early in 1942 at the suggestion of Briggs. The experimental program centered at the National Bureau of Standards and the Geophysical Laboratory. To this end a standard 3-inch gun was set up in the spring of 1942 at the Navy Department's David Taylor Model Basin at Carderock, Maryland, and the investigations outlined in Chapter XXXVI were performed there.

These experiments became an outstanding example of the application of precise physical measurement to ballistics. The data obtained have wide applicability to gun and projectile design, and provide a sound basis for verification of ballistic theories. After three years so many new problems in interior ballistics had arisen that the division recommended a new series of experiments, with new procedures, to aid in developing a consistent theory of interior ballistics. The Navy Bureau of Ordnance supported a continuation of this work, both the experimental phase by the National Bureau of Standards and the theoretical by the Catholic University.

By the summer of 1942 the protagonists of hypervelocity had developed the major fronts for attack. Each held promise of overcoming one or more of the obstacles to high velocity. The advancing columns might reach the objective more or less together, or far apart. At the June 18 meeting of the section the advance was organized along these four general lines: (1) erosion studies; (2) erosion-resistant materials, which are described more fully in Chapter XXXIV; (3) sabot projectiles, discussed in some detail in Chapter XXXV; (4) tapered-bore gun, details of which will also be found in Chapter XXXV.

On December 9, 1942, with the major reorganization of the NDRC setup, Section A–A became Division 1, the "Division of Ballistic Research." The new division had no sections. Some personnel changes were made to effectuate the reorganization.

Up to this time expanding the forces had been the paramount consideration. Twenty contractors were then employed in designing, fabricating, or processing specimens of various kinds; conducting a number of ballistic experiments; designing special projectiles such as the sabot and pre-engraved projectiles. One contractor was engaged in developing a new automatic 20-mm. cannon for aircraft.

Other contractors were added to the ranks from time to time, until 38 separate contracts in all were being carried on by 29 different laboratories and concerns, one as far away as Los Angeles. The funds allocated eventually aggregated not far from $5,600,000.

The division found one OSRD instruction particularly useful in its dealings with contractors:

Contractors must also be given a free hand to work out their assigned tasks in accordance with their own conception of what should be done. . . . The research scientist, is, after all, exploring the unknown. He cannot be given detailed instructions as to the solution of the problem which he is retained to solve. If that could be done, there would be no need for the research scientist.

This farsighted policy proved a powerful stimulant to ingenuity and invention; with suitable direction and co-ordination the contractors were able to proceed steadily toward solution of problems that sometimes seemed all but insoluble when work was initiated. Controls set up during the next few months grew into an intricate network of reviewing committees and steering committees which are described in Chapter XXXIII. Division and committee meetings became frequent, generally once a month.

As every investigator worked steadily in his laboratory, office, or shop he was granted the utmost possible freedom. The work of collaborators, if it affected his work, was reported to him at least once a month, and similarly, his own work was described, summarized, and analyzed. Close supervision never for a moment lost sight of the short-range results demanded, or the long-range studies that were to make them truly effective.

The 7th of July, 1943, was a red-letter day for Division 1, when a short liner[10] in the breech end of a .50-caliber machine-gun barrel yielded some surprising results at the Geophysical Laboratory firing range. This liner proved extremely resistant to test firing. *No gas erosion* could be detected; however, as the material was unhardened, the gun's "lands" (the bore surface between the rifling grooves) were swaged, or flattened down, under the impact of the driving bullets.

This was a vitally useful observation, because now a clear distinction had been noted between gas-produced erosion and land swaging; these were found to be the two major factors in deterioration of the machine-gun barrel. It pointed a definite direction in the search for practical gun liner materials, a turning in the long lane of laboratory research, which had involved hundreds of firing tests and erosion measurements.

Following this "lucky break" in the laboratory the development of erosion-resistant materials followed several clearly defined paths. One of these dealt with the development of what will be called "Alloy X," which was evolved as a hard liner material from the soft metal that had been used in the test described above. Another led to Stellite-lined machine-gun barrels. A third concerned the development of improved electroplates for the gun-bore surface and a fourth, the development of various other alloys for use as liners. Each of these developments is recounted in Chapter XXXIV.

[10]Made of a material previously tested at Aberdeen Proving Ground, but not produced in suitable form for gun liners.

This resistant-materials program involved the close co-operation of some 15 of the division's contractors. The "spark plug" who fired the enthusiasm of these contractors — even when their efforts seemed to be of no avail in making refractory metals behave — was Dr. J. F. Schairer.[11] Through his tireless efforts which involved spending half his time traveling from one laboratory to another, each one of this group of contractors was kept fully informed of the progress being made by the others. Through his grasp of the ramifications of this exceedingly complex metallurgical program, he was able to pick up information at one laboratory and relay it to another one which could use it.

Nowhere was this more true than in the development of "Alloy X." Making a workable liner material from what initially was a brittle and intractable substance required the combined efforts of Westinghouse Research Laboratories, Westinghouse Lamp Division, the Franklin Institute and the Geophysical Laboratory. This remarkable alloy came into being only after new processes and improved manufacturing techniques had been painstakingly evolved.

The division's "organized mass attack" on erosion, the hidden gunbore enemy, produced startling results — and, as had been hoped, "in time for this war." Compressing into months the research which normally would have taken years, the division's vigorous, even unorthodox methods and its fresh ideas resulted in progress in one direction that culminated in a notable demonstration of a 6-inch liner of a Haynes Stellite alloy inserted in a caliber .50 heavy machine-gun barrel.

On the firing range this liner amazed the testing crew as it withstood burst after burst, 100 rounds at a time, with only a minute's cooling between bursts. They found the barrel containing this liner could even be fired until red-hot, then cooled, and fired again with accuracy practically as good as ever. This was almost unbelievable service compared to the gunsteel barrel then in use.

Wherever men came to grips, the excellent — but short-lived — caliber .50 machine gun was in the thick of the fight. Good as it was, it still did not meet the fighting man's ever-rising demands on land, on the sea, or in the air. As the machine gun assumed increasing importance, Division 1 felt impelled to concentrate not a little of its resources upon this vital gun, in the effort to augment its power and extend its life. The Stellite liner that had lasted so long pointed the way to worth-while improvement of the caliber .50 barrel.

Shortly after the history-making liner test, mentioned above, a division conference held at the Crane Company in Chicago turned into an idea-

[11]A member of the Geophysical Laboratory staff who had held a position as Consultant to the division since 1941, and who later became a full-time Special Assistant.

crystallizing session of great importance to the division and its war effort. As a result of the showing Stellite had made, the division made the vital decision following Rose's recommendation to "freeze" the liner design for an aircraft barrel on the basis of tests then underway, and to produce 200 barrels equipped with these liners, for immediate demonstration to the Armed Services. The liner castings were made by Haynes Stellite Company.[12]

These lined barrels proved to be as good as represented. In September 2000 Stellite-lined barrels were ordered, to be rushed to active theaters for combat test. Some production troubles remained to be beaten, but in the end — and in time to help many a gunner in the Pacific theater — every obstacle was overcome. The Stellite liner proved a phenomenal solution to the machine-gun barrel's major weakness. Some of the new barrels were flown to Saipan, and there were demonstrated to astonished aircraft gunners. By the end of 1944 many thousands of the modified barrels were contracted for by the War Department, which had adopted them as standard.

By the time that the first of these Stellite-lined barrels were being produced, another means of increasing the life of the caliber .50 machine-gun barrel had also been brought to the stage of Service testing. This improvement, which had been developed co-operatively by the National Bureau of Standards and the Geophysical Laboratory during two years of extensive testing, was the application of chromium electroplate on the bore surface after the latter had been hardened by nitriding. A several-fold increase in the life of the barrel when fired in long bursts was found to be possible if the thickness of the chromium deposit was tapered so that the bore of the gun was choked slightly at the muzzle.

Both the Stellite-lined and the nitrided and chromium-plated caliber .50 barrels were adopted as standard by the Ordnance Department in January 1944. Later Division 1, through the co-operative efforts of the Geophysical Laboratory, the National Bureau of Standards, and the Crane Company, was able to offer to the Services an even better barrel that combined a Stellite liner with choked muzzle chromium plate. These barrels, which could withstand thirty times as many rounds as would ruin ordinary steel barrels fired on the same schedule, were about to be produced on a large scale when production contracts were canceled by the Army immediately after V–J Day.

On January 22, 1944, the NDRC Reviewing Subcommittee for Division 1 realized that the major exploratory and experimental phases of its work were nearing the end. The committee therefore recommended that emphasis be shifted from research to engineering development. J. A. TenBrook, obtained on leave from the Philadelphia Electric Company, was assigned

[12]Subsidiary of Union Carbide and Carbon Corporation.

by the Engineering and Transition Office of NDRC, to serve as full-time engineering adviser to the division.

Plans for a new organization, involving a separate operating office in Philadelphia,[13] under Allen, the Deputy Chief, were put into effect October 1, 1944. This relieved Adams of some of the burden of supervising a program then proceeding at a cost of about $200,000 a month. Allen's three Branches were Research, Engineering and Development, and Administrative. The Research Branch was headed by Burlew; the Engineering and Development Branch by TenBrook, who was succeeded in the spring of 1945 by W. H. Shallenberger of the NDRC Engineering and Transition Office.

As part of this reorganization two new division members were added, especially to aid in supervising the committee activities. These Members were Dr. C. E. MacQuigg, Dean of Engineering at Ohio State University; and Dr. Rupen Eksergian, Chief Consulting Engineer, E. G. Budd Manufacturing Company, who had been a Division Consultant since May 12, 1942. Seven Project-Control Committees, one for each major division project, took over the work of the former Steering Committees.

Although the division was deeply gratified by the outstanding successes achieved with its improved machine-gun barrels, at no time did it lose sight of its primary objective — elimination of the obstacles to hypervelocity in larger-caliber guns. Gun barrels, both large and small, wear out because of erosion. Division 1's fundamental studies of the causes of this phenomenon had led to the recognition of how those causes operate differently, depending upon the temperature condition of the barrel. These differences are so striking that in effect there are at least two problems of gun erosion — the "machine-gun problem" and the "large-gun problem."

In the case of machine guns and other small arms, erosion by the powder gases has been found to be slight. Because of the rapidity with which machine-gun barrels are fired they soon become red-hot. Then the steel loses its strength and the lands of the rifling quickly become flattened under impact by the projectile, and thus the barrel may be ruined by a single long burst. Stellite is the answer to this problem; chromium plating on a nitrided bore surface is equally good under certain circumstances; and the combination of a Stellite liner and chromium plating is much more effective than either alone.

In larger guns, which are not fired so rapidly as machine guns, the temperature of a thin layer of steel at the bore surface becomes very high during the firing of even a single round. The white-hot powder gases melt thin films of metal, which are then blown away by the rush of the gases. This slow attrition by powder-gas erosion ruins the rifling, particularly at

[13] Located in the building of the Franklin Institute.

the beginning of the bore just ahead of the powder chamber. In order to combat this condition a bore surface material must have a high melting point as well as inertness to chemical action by the powder gases.

The super-gun has to withstand a combination of hypervelocity with rapid fire. Erosion is thereby intensified to such an extent that most materials fail, although satisfactory under either condition alone. Alloy X is so resistant, however, that it bids fair to be able to meet this requirement and thus make possible super-guns ranging from machine guns to medium-caliber artillery, all having higher muzzle velocities than would have been dreamed possible a few years ago.

By the beginning of 1944 the mounting intensity of land, sea and air combat had ended all debate as to the urgent need for higher muzzle velocities. The division had made steady progress toward its objectives, having developed six distinct solutions to the problem of how to attain hypervelocity without sacrificing barrel life.

So well advanced and practical were the devices developed by the early part of 1944 that every one had been or was about to be offered to the Services, for application to their weapons. All six developments were productive of, or conducive toward higher muzzle velocities; each is dealt with seriatim in the chapters which follow: the basic methods included replaceable short steel liners, erosion-resistant liners and coatings, the Fisa Protector, the sabot projectile, the tapered-bore gun, and the pre-engraved projectile.

By the spring of 1944, when erosion and other studies were well advanced, it was felt that the division might well incorporate in a gun of medium caliber all its ballistic knowledge, and at least some of its various anti-erosion techniques, such as gun-bore coatings and liners. A special committee of division members headed by Rose made a thorough study of the project, even considering the possibilities of a gun with 6000 feet-per-second velocity for antiaircraft use.

Discussion during succeeding Division Meetings revolved around the question of whether such a super-gun could be developed in time for use during the war. The conclusion was that it might, the decision taken to go ahead.

Ideas crystallized on a muzzle velocity of 4000 feet per second as the goal for a 90-mm. gun, termed the A–Z Project. A Project-Control Committee was appointed, NDRC approval was obtained, money set aside, data analyzed, and definite plans laid.

The project was moving along steadily when the war ended. Ballistic calculations were completed, designs made for a suitable pre-engraved projectile, a number of gun tubes had been forged by the Midvale Company, and machining had begun. In September 1945, as the division started

terminating its long and difficult undertakings, the A–Z Project was taken over by Army Ordnance Department.

At the end of its expedition Division 1 was gratified in being called on to transfer a number of its projects which were to be continued by Service agencies. The division carried on some investigations after conclusion of hostilities, in order to reach a satisfactory stopping place. As is so often true in research, some of its projects, such as those in interior ballistics, never can be considered complete, but must be carried on as long as improved guns are required.

All the division projects have been put in such shape that they may be picked up and carried forward at any time in the future. In this respect future investigators will have much less trouble than was encountered by Division 1 in its studies of work that had been dropped immediately at the end of World War I.

In order that a clear picture may be left as to the state of the art at termination of World War II, careful attention was given to final reports; nearly 150 of these have been co-ordinated in a Summary Technical Report, which is to be turned over to the Services as a guide in any future program of research in the field of erosion and hypervelocity.

Division 1 ended its program feeling that it had made important pioneering advances into the field of fundamental gun-barrel reactions, a field in which there had previously been only limited investigation. Demonstrating the resourcefulness of scientists in unfamiliar spheres, its ballistic studies have laid the foundation for a much closer approach to "exactness" in calculation and in barrel design. Its investigations have uncovered the chief factors that cause erosion; these have been evaluated, and countermeasures have been developed.

New gun-barrel materials and propellants can now be rapidly appraised in relation to gun erosion. Their utility under different firing conditions, including those of very high velocity, can be determined with some precision.

With this basic knowledge super-guns can be designed and constructed in every size and type required for the nation's defense. With the bore made erosion-resistant by one of the means developed by the division's investigators, the new type of gun will be by far the world's most powerful, as well as the most durable.

A source of great satisfaction to the division was the final acceptance of its hypervelocity objectives for all sizes and types of guns. After a general conference held July 19, 1945, Adams reported:

"The Ordnance Department appears now to have a keen interest in hypervelocity guns and projectiles, and looks forward to employing generally muzzle velocities of 4000 feet per second or more."

Succeeding chapters of this history deal more closely with explorations of

the hypervelocity field by the division[14] in its five-year quest for the fundamental elements with which the super-gun of the future could be built.

[14]*DIVISION 1 ROSTER*[a]

Adams, L. H., CHIEF,
June 1941 — June 1946.
[b]Allen, H. B., DEPUTY CHIEF,
May 1942 — June 1946.
[h]Bainbridge, E., ENGINEER,
October 1944 — May 1946.
Black, H. L., TECHNICAL AIDE,
May 1945 — May 1946.
Bleakney, W., MEMBER,
January 1943 — March 1946.
Briggs, L. J., MEMBER,
October 1941 — June 1946.
[e]Burlew, J. S., SECRETARY,
December 1942 — October 1944; TECHNICAL AIDE, November 1944 — June 1946.
[c]Eksergian, R., MEMBER,
September 1944 — June 1946.
[f]Greig, J. W., CONSULTANT,
November 1941 — September 1944.
Hart, Grace L., TECHNICAL AIDE,
October 1943 — June 1944.
Kneen, O. H., TECHNICAL AIDE,
April 1946 — June 1946.
Line, L. E., Jr., TECHNICAL AIDE,
March 1943 — May 1946.
MacQuigg, C. E., MEMBER,
September 1944 — June 1946.
Marble, J. P., TECHNICAL AIDE,
June 1942 — March 1945; SPECIAL ASSISTANT, March 1945 — June 1946.
[d]Rose, E. L., MEMBER,
June 1942 — June 1946.
[g]Schairer, J. F., SPECIAL ASSISTANT,
October 1944 — June 1946.
Schubert, Rita G., TECHNICAL AIDE,
June 1945 — February 1946.
[h]Shallenberger, W. H., ENGINEER,
May 1945 — March 1946.
Smith, N. H., TECHNICAL AIDE,
July 1942 — April 1945.
[h]TenBrook, J. A., SPECIAL ASSISTANT,
March 1944 — October 1945.
Watson, Helen M., TECHNICAL AIDE,
October 1943 — June 1946.
Weidlein, E. R., MEMBER,
October 1941 — June 1946.
Wichum, V., ENGINEER,
October 1944 — February 1946.

Others who served as Consultants were P. W. Bridgman, Harvey L. Curtis, Gordon S. Fulcher, R. E. Gibson, O. A. Hougen, Wendell F. Jackson, R. S. McBride, Leo Nordheim, and F. C. Walcott.

[a]Dates of appointment prior to December 1942 apply to Section A, Division A, from which Division 1 was formed in the reorganization of NDRC.

[b]Held appointment as Member from March 1942 to May 1942.

[c]Held appointment as Consultant from May 1942 to September 1944.

[d]Held appointment as Consultant from March 1942 to June 1942.

[e]Held appointment as Consultant from July 1941 to December 1942.

[f]Title of appointment was "Consultant," but functions were those of a Special Assistant.

[g]Held appointment as Consultant from November 1941 to December 1942, and from July 1943 to October 1944.

[h]Assigned by Engineering and Transition Office, NDRC, to Division 1.

## CHAPTER XXXII

# *ORGANIZING THE ADVENTURE*

AS AN OBVIOUS scientific preliminary, a thorough survey of the previous investigations of gun erosion was begun in the summer of 1941 by Burlew, who, searching Army and Navy files, noted the landmarks of a century-long evolution. The information he collected was communicated piecemeal as obtained to his colleagues so that they might quickly master enough of the art of ordnance and ballistics to plan intelligently. Later he prepared a report,[1] "The Erosion of Guns," which served as a summary primer for each new investigator. The following brief account of what had been known about the likely causes of erosion has been based upon it.

As propellant powder burns in a gun chamber, which at first is sealed off by the projectile, the force exerted by rapid expansion of the gases expels the projectile from the muzzle. In larger guns the projectile is encircled by one or more narrow "rotating bands." Made of soft metal, these bands engage with spirally rifled grooves in the gun's bore surface, to give the projectile a spin which assures stabilized flight. The rotating band also seals the bore so that the high-pressure gases do not escape ahead of the shell. In machine guns the gliding metal jacket of the bullet takes the place of a separate rotating band.

Erosion, believed to begin with the first shot fired from a new gun, is essentially the gradual removal of thin layers of metal from the bore surface. This enlargement of the bore, chiefly at the breech, radically affects the projectile's flight, decreases the gun's range and its accuracy. The principal causes are the action of the hot powder gases, and mechanical wear produced as the projectile grinds against the bore surface.

Long before Burlew had completed his study, the NDRC men knew that they were treading the borders of a discouragingly complex field. The possible reasons for erosion were legion.

Any of the following might affect erosion, perhaps to an important degree:

*The composition of steel* or alloy used for the gun tube. Metallurgists and ordnance men had made a great many firing tests of various steels, and the specifications used by Army and Navy were believed to be the best obtain-

[1]NDRC Reports A-90 and A-91.

able for the combination of strength with resistance to the effects of high temperatures.

*The state of stress* in the gun tube, produced by heat treatment or cold-working during fabrication. No serious fundamental research had been done on this point.

*Rifling:* Various erosive and wearing effects were perhaps caused when the lands "engraved" the rotating band. The amount of twist of the grooving would perhaps affect the rate of bore erosion.

*Chamber design:* Some authorities believed that erosion could be decreased if the diameter of the gun chamber were made not much larger than the bore of the gun. There was no convincing proof for this.

*The rotating band:* Was the frictional wear of the rotating band and of the "bourrelet" (the forward part of a projectile which rides on the lands) mainly responsible for gun-bore erosion? Some authorities thought so. Possibly the material of the rotating band — ordinarily copper or a copper alloy — had an important bearing on erosion; some tests had been made with other materials, and some friction measurements had been made. This friction had been found to be greatest, proportionately, in small arms. No attempt had been made to measure directly the heating of the bore surface caused by friction during firing. There was evidence that gases leaking past the projectile scoured the bore surface; a change in design of the rotating band might mitigate this.

*Propellants:* The chemical and other erosive effects caused by the high temperatures and high pressures of the powder gases were not well understood, mainly because of the difficulty of measurement, in view of the extremely brief interval the projectile takes to reach the muzzle, and the experimenter's inability to hold a number of factors constant while varying each factor being studied. Relatively cool powders had been sought, presumably with less erosive effects and less "flash" from the muzzle after firing.

*Expansion of powder gases:* There was an hypothesis that expansion and cooling of the gases in a gun might promote some reaction affected by the changing pressures, perhaps forming a particularly erosive combination.

*Gun-barrel temperatures:* Careful measurements made on artillery served as the basis for setting maximum allowable rates of fire; firing at higher rates eventually raised the gun-barrel temperature so high that its tensile strength decreased to a dangerous degree. The exact erosive effect at the bore surface, which becomes very hot because it does not have time to cool appreciably, had not been determined.

*Cracking:* Severe thermal stresses result when sudden heating is followed by sudden cooling; cracking of the inner bore surface had often been observed. This might be a primary cause of erosion or it might facilitate chemical reaction beneath the surface, in the "altered layer." Existence of a

complex system of several very thin layers had been discovered by microscopic examination of cross-sections.

These and similar unsolved questions were constantly arising during the preliminary survey of gun erosion. Authorities were not even agreed as to *how* the erosion occurred — whether the metal was actually melted off the bore surface, or whether some physical or chemical agent, or combination, loosened part of the surface metal, for removal by the same or another agent.

Burlew pointed out that all that preceding investigators had agreed to was that erosion was a "dynamic phenomenon," occurring only when the bore surface was attacked by hot powder gases in motion. To secure a true explanation of erosion, he concluded, it would be necessary to understand *in detail* all the important physico-chemical changes undergone by the bore material as it reacted during firing.

As planning of the gun-erosion project progressed, it became apparent that the Geophysical Laboratory and its trained investigators were well fitted for this incursion into the unexplored fields of ballistics. Basic training in geological, chemical and physical branches of science turned out to be a useful preparation for fundamental research into the causes and cure of gun erosion.

For example, some of the techniques developed by the geophysicists during heat conduction studies were readily adaptable to measure the flow of heat in gun barrels. This was a basic problem in the study of barrel erosion. Some investigators at the Geophysical Laboratory had done special work in microscopic identification of materials, a foundation of petrology and study of the earth's structure. This technique became vital in tracking down and isolating the products of erosion, and thus determining just how erosion occurs under all conditions of firing.

To measure, record and analyze complex reactions in a gun called for application of advanced laboratory practices. Others on the staff devised and applied special techniques to high-temperature and high-pressure studies. The Laboratory's own researches had dealt with hydrostatic pressures ranging up to 1,500,000 pounds per square inch, and so the 60,000-pounds-per-square-inch pressures in the gun barrel did not seem impressive, nor was a temperature of 3000 degrees Centigrade, as found in some gun chambers at the height of the explosion.

The Geophysical Laboratory had for many years taken a leading part in the identification of minerals and the study of their structure by X-ray examination. This knowledge was useful in the study of eroded specimens of bore surfaces, work which was to lead to important discoveries.

The adaptability of the geologist no doubt stemmed from his versatility. Often engaged in activities that cross the boundaries of the various sciences, particularly physics, chemistry, and geology, his ability to produce results

in a strange field was never doubted by the planners of wartime research.[2]

When the geophysicists needed help, as the program was accelerated, the staff was augmented by persons with appropriate special training. Thus a metallurgist was found necessary as soon as the development of erosion-resistant materials became a major phase of the laboratory's efforts. Also, mathematics, another "basic branch of science," contributed several to the Geophysical Laboratory's war staff.[3]

Frank discussion, fact, opinion, optimism and the reverse, enlivened the Watertown conference, held October 15, 1941. This meeting of six NDRC "amateurs" with thirty ordnance officers and civilian specialists occurred at one of the nation's oldest ordnance centers — Watertown Arsenal, Watertown, Massachusetts.

Among the more hopeful (outside of NDRC) was Brigadier General R. W. Case of the Watertown Arsenal, who stressed the "almost infinite possibilities" and utility of an erosion research project, which, if successful, would permit increased muzzle velocities and thereby improve gunnery. Other officers agreed that answers would help but, being less sanguine about getting them, rated the odds differently.

For the Navy, Captain G. L. Schuyler remarked,[4] "We have been at it pretty intensively for about 30 years. But erosion studies are now relatively inactive in the field of trying to *prevent* erosion."

[2]The National Resources Board's "Research — A National Resource," calling attention to geology as a composite and pioneer science involving many subjects and novel techniques, referred to the great value of these "border-line fields of research." In such fields, it was noted, physics and chemistry play leading parts, and workers co-ordinate and utilize specialized knowledge from more than one field. Lauding this as "one of the most important modern trends in modern research," the report concludes: "There is little doubt that in many respects the worker in border-line fields represents the spear head of research. The consolidation and coordination of scientific information from many fields and the welding of it into a powerful new tool to attack new and important regions of the unknown has always been a tendency of any youthful human endeavor. The worker in border-lines is a pioneer, and as such an immense national resource."

[3]Realizing that few outside the "charmed circle" of research workers have a clear understanding as to how the mathematician fits into the investigations of natural phenomena, the National Resources Board in 1940 made an analysis of which the salient points are given below; it serves to indicate how essential mathematicians were to Division 1's difficult problems in ballistics and other phases of gun-bore study.

The mathematician feels great confidence in a conclusion reached by careful reasoning. The work of the mathematician is dominated by greater confidence in logical lines than in experimental proof, severe criticism of details, idealization, and generalization. These traits are of highest importance in the job of finding a system of thought which will harmonize the complex phenomena of the physical world, that is, in reducing nature to a science. Modern molecular physics plays an important part in determining the speed of reactions.

As research becomes more complex and theoretical, the value of the mathematician increases. Because of his training in exact thinking he should be better able to see through the maze of intricate details and discover the fundamental problems involved. When data are incompatible with the preconceived theory, a mathematical study frequently aids in perfecting the theory itself.

[4]Then Director of Division of Research and Development, Navy Bureau of Ordnance.

Discussion revealed some of the reasons for this paradoxical "relative inactivity" in a line of research admittedly of "tremendous importance." The reasons are evident in remarks made by various conferees such as:

"We can't really get a lot of 16-inch guns and fire them to destruction."

"There seems to be no way in which we can reproduce in the laboratory a sufficiently good simulation of what goes on inside such a gun."

"The Navy is not interested in small-caliber, terrifically high-velocity antitank guns."

"Recently we have not been able to think up projects connected with erosion which we thought we could usefully undertake for the present emergency."

"We would be interested in experiments; but the machinery for a useful experimental research so far seems lacking."

The conference brought out that research in erosion had not progressed much beyond the cut-and-try stage — that is, the so-called "Edisonian" approach. In other words, "trying everything anyone could think of, to find out if anything suitable for gun construction worked." [5] It was emphasized that a vast number of firing tests had been made, during which the erosion had usually been measured and recorded.

A long series of studies had been made to improve gun steels, and to a minor extent the propellent powders, with some useful results; some experiments had been made on heat checking of the bore surface. Design of projectiles and their rotating bands had been explored from the viewpoint of erosion. Other physical, metallurgical, and, to a lesser extent, chemical reactions in the gun barrel had been the subject of experimentation. Vent plug tests had been made of gun steels and other materials; these tests were useful in that they avoided the need for proving each new material in an actual gun, as metallurgists continued their search for the "ideal" gun-barrel material. Vent plug tests of the past, unfortunately, had not given reliable results as to the gun-barrel performance of a tested material. An improved vent plug test eventually enabled investigators to reject new materials at an early stage, permitting concentration on more suitable ones.

[5] There was the method, for example, of placing disks of various materials in the gun chamber with the propelling charge, and analyzing them after the gun had been fired, to study erosion effects. Useful to some degree, this method did not produce any evidence of fundamental importance because, as Dr. R. E. Gibson of NDRC pointed out:

"When you are through with the plates you are not much farther forward than at the beginning. You know that something in that chamber at some temperature and pressure has made those plates look as they do, but you have so many variables to pick from and so many variations of pressure and temperature that you cannot state specifically what has caused the reactions."

Although guns were being tested to destruction (a common practice being to cut worn guns apart for examination) and vast files of firing and erosion statistics had been compiled at the proving grounds, the ordnance specialists had been unable to determine what actually occurs when the bore surface erodes under fire. They were, therefore, admittedly at a loss to know what steps to take in order to bring these erosion-causing factors under control.

The conference, which the section had taken an important part in initiating, served some valuable purposes. It afforded the ordnance specialists a rare opportunity to air their various and divergent theories as to the possible causes of gun erosion. The discussion revealed clearly that there was no general agreement on even the major ones. But the need for a comprehensive program of research was admitted by all.

Vitally needed, the NDRC men reiterated, was a "quantitative appraisal of the role that each factor may play in gun erosion." In other words, they felt that they should devote attention "to consideration of the basic mechanism of erosion."

The newcomers to the study of gun erosion were going to undertake scientific, long-range studies and at the same time to test all kinds of promising devices and methods, as for example plating the bore surface, which might reduce erosion and thus help the ordnance engineer to increase muzzle velocities.

It was indicated at one point that NDRC had the rather startling ambition to reduce erosion to a mere 10 per cent! And it was conceded that "cutting out 90 per cent of the erosion would do wonders."

The genesis of Section A–A's program was explained by Adams as follows:

By way of background, it may not be inappropriate to say a word about the interest of our group in this subject and the way we fit into the picture. Early in the summer (1941) the NDRC committed itself to a comprehensive program on gun erosion as a factor which limits high muzzle velocity. That was done in the belief that it was a fundamentally important matter for improvement, especially in antiaircraft and antitank guns; and also in the belief that no really comprehensive program of research on gun erosion had ever been carried out.

The action was taken with no implied criticism of the excellent work that has already been done, but rather with the knowledge that, despite the interest that has been shown in the subject and the full realization of its importance, the limitations of funds and of working space have prevented the development of investigation in this field to the extent which its fundamental importance justified.

It was set up as a long-range program without, however, any restrictions on trying any individual item for immediate application, or 'playing the hunches' if they seemed to have reasonable merit. Section A was created in Division A to assume the responsibility.

It must be realized that this is a complex field, discouragingly complex, and

that a vast amount of work has been done at various places in the past. In normal times, there might have been much more hesitation about plunging into something with so uncertain a future.

There is no question but that the idea was advanced without an opportunity for thorough consideration, but the thought was that by putting behind such a venture a great deal more in the way of resources and facilities, we could advance it farther and faster than would otherwise be possible. That could be done, for example, by co-operating with the existing agencies interested in that kind of work, finding means at one place or another to supplement those efforts that seemed most desirable.

Although some elements of the long-range program proposed by NDRC were generally approved, the Services appeared to be pessimistic as to the achievement of any early results. Perhaps all of the officers did not entirely agree with the discouraging remark of one old-time ordnance man: "It is impossible to prove in advance that any particular erosion project taken up is going to be entirely a waste of time. But that is still my general feeling in spite of our gratefulness for the kind offers to undertake various projects for us in erosion."

Some, no doubt, felt inclined to agree with a riposte by Gibson of NDRC to the following comment: "I think some of these things proposed in the outlined program are out of reach. The measurement, for instance, not only in time but in space, of the instantaneous temperature and pressure inside a gun sounds to me like an insuperable job."

To which Gibson replied: "That is about the only kind of job worth taking on."

In spite of the difficulties every NDRC man left the Watertown conference still confident that trained investigators adequately supported could accomplish what cut-and-try methods had not succeeded in doing — solve once and for all the erosion problems of the gun barrel — and "do something about it." NDRC's decision was to carry out the project with all the vigor and resources at its command.

During October 1941, Weidlein and Briggs were chosen as the first two members[6] of Section A–A's "high command." [7] Following their acceptance the remaining five members of Section A–A were appointed from time to time as follows:

Edwin L. Rose, Director of Research for Jones and Lamson Company; appointed Consultant March 14, 1942, and member June 4, 1942.

Dr. Henry B. Allen, Secretary and Director of the Franklin Institute;

[6]The functions of a Section Member corresponded to those of a Division Member as described in paragraph 2 of the letter quoted in Appendix 6.

[7]In a letter to the chairman of Division A, Dr. Adams explained his basis of selection, as well as the outstanding qualifications of the two leaders of science in their respective fields. This letter appears in Appendix 4.

appointed member March 20, 1942, and Vice-Chairman of the section May 22, 1942.

At later dates three others became members of Division 1:

Dr. Walker Bleakney, Associate Professor of Physics at Princeton University and Deputy Chief of Division 2; appointed member January 1, 1943.

Dr. Rupen Eksergian, Chief Consulting Engineer, E. G. Budd Manufacturing Company, Philadelphia; appointed Consultant May 12, 1942, and member September 15, 1944.

Dr. C. E. MacQuigg, Dean of Engineering at Ohio State University; appointed member September 15, 1944.

The division was fortunate in obtaining the services of these seven section and division members. Each was busily employed; some were carrying on urgent defense activities. Weidlein, for example, in addition to his regular duties as Director of the Mellon Institute, was devoting much of his time to the synthetic rubber program. Briggs, in addition to being Director of the National Bureau of Standards, was Chairman of the first committee that was looking into atomic bomb research. But all gave time freely and unreservedly. In fact, they took their responsibilities so seriously that they had a record of nearly 100 per cent attendance at the forty-odd section and division meetings which began with the executive session of December 3, 1941.

As rapidly as possible the section's program was broken down into different phases suitable for separate contracts. Therefore, during the balance of 1942, the section moved to spread out its contracts. Every month of that year, except December, one or more contracts was awarded.

During that first "organizing year" of 1942 twenty contracts were awarded, the largest for any single year of the program.[8] They covered a wide field of research involving fundamental studies; preparation and testing of erosion-resistant specimens of alloys, and platings; development and manufacture of special projectiles such as the subcaliber (sabot) types; and design of an automatic aircraft cannon. Special studies were undertaken in more abstruse subjects requiring mathematical analysis, such as centrifugal effects in projectiles; burning rates of propellants; bore friction and other features of interior ballistics.

Thus the program embraced the two often-reiterated approaches to the problem. In addition to attempting the development of new devices by applying well-known principles, a broad foundation of basic research was established. To this policy much of the success eventually achieved by Division 1 can be attributed; for not only did the basic research give results that could be translated into action, but also it clarified the efforts of those already at work on improved weapons.

[8] A list of the contractors and a brief description of the work done by each one appears in Appendix 1.

The first allocation under Section A–A's 1942 budget of $200,000 was for an increase in the funds available under the contract with the Carnegie Institution of Washington for work at the Geophysical Laboratory, to provide for the hiring of additional investigators, instrument makers, and clerks and the purchase of equipment and supplies. Soon nearly all the resources of the Geophysical Laboratory were focused on the new problem.

While Adams, Greig, and Burlew considered the many suggestions for studying gun erosion, in order to determine the assignments to the other members of the staff, the latter were clearing the decks. At a staff meeting on February 12, Adams as Director of the Laboratory announced the following project outline, after which personnel were assigned to all but one of the fifteen outlined phases of the subject.

I. Summary of the literature of the subject
II. Ballistic calculations
III. Examination and identification of specimens . . . for obtaining a clue to the chemical and physical changes that take place in the surface of the bore during firing . . .
IV. Study of the chemical reaction of gases with steel at high temperatures and pressures . . .
V. Calculation of the composition and temperature of the powder gases and of the temperature of the bore surface
VI. Experimental determination of the composition and temperature of the powder gases
VII. Determination of the thermal conductivity of the altered layer formed on steel by hot powder gases
VIII. Preparation of a piezoelectric gauge with oscillograph for measurement of the pressure of powder gases
IX. Experimental study of the effect of stress on erosion
X. Development of an erosion plug apparatus . . .
XI. Development of an apparatus for collecting the products of explosion from a rifle . . .
XII. Development of an apparatus for comparison of the leakage of gas past the projectile . . .
XIII. Development of a bore microscope . . .
XIV. Apparatus for measuring the rate of erosion . . .
XV. Trial of preventives against erosion . . .

Projects III and X later became subprojects under the broad program to develop erosion-resistant materials and means for improving the caliber .50 barrel. Project XIV was not begun.

Although the above outline was primarily for work to be carried on at the Geophysical Laboratory, it was also the erosion project outline for Section A–A up to that time. Thus the work under the contract just arranged with

Bell Telephone Laboratories was an adjunct to Part III of the outline, and it was planned to obtain assistance from the National Bureau of Standards for this part. It was also planned that other contractors would be brought in to supplement other parts of this outline. Thus the contract subsequently arranged with Johns Hopkins University related to Part IV of this outline.

During the first six months of the existence of Section A–A its activities were confined to the Geophysical Laboratory. It had been recognized from the beginning that the Laboratory could not possibly care for all of the investigations which came to be involved in this all-out attack on major ordnance problems. It was necessary to utilize laboratory and production men with varied training and background. The urgency of war, and the difficult nature of the problems, demanded as many hands and heads as could be put to work along the many paths into the unknown.

At first this additional help was arranged to supplement certain phases of the investigations already underway there, as by the contract with Bell Telephone Laboratories. By the latter part of March 1942 a fundamental decision had to be made as to expansion of the Section's activities. It now became necessary to decide whether to set up other contracts having objectives paralleling some included in the Geophysical Laboratory's broad contract, so that the program could be advanced independently in several directions at once, with co-ordination provided by the staff of Section A–A.

The basic decision to be made concerned the status of the Geophysical Laboratory. Some advocated that the Laboratory, as principal contractor, should be made the operating agency for the entire project, as had been done in some other sections of NDRC. Under such a plan, work to be "farmed out" to prime or subcontractors would be directly supervised by the Laboratory, which would thus be the responsible unit for all erosion and hypervelocity investigations by NDRC.

An alternative plan was for the section, as a Government agency, to supervise directly all the contractors. The Geophysical Laboratory under this plan would be one of the prime contractors, but one which served as a focus of the whole program and tied in closely with most of the work of other contractors, although primarily it assumed responsibility only for its own part of the program.

For various reasons the latter plan was adopted. The Laboratory's work as contractor was defined, and the section's offices and administrative duties were separated in the main from those of the Laboratory, although for convenience the main section office continued to be located in the Geophysical Laboratory building.

In carrying out this plan the change was made gradually, for many of the staff members of the Laboratory were helpful in finding other contractors and in getting them started on specific problems. After Section A–A had been reorganized as Division 1 in December 1942, the appoint-

ments of these staff members as Consultants were not renewed, and Adams took steps to avoid "overlapping interests." These plans were completed by March 1, 1943.

In a memorandum to members of the Geophysical Laboratory staff on that date, Adams pointed out that, while from then on the Laboratory was to be considered merely one of the twenty-two contractors carrying out the technical program of Division 1, the location of the Division Office at the Laboratory, which also was the focus of a large part of the program, should not cause confusion. He noted further that Bush, as Director of OSRD and also as President of the Carnegie Institution, had requested that as sharp a line as possible be drawn between this contractor's affairs (or private business) and NDRC affairs (or Government business). This memorandum therefore proclaimed a complete separation of the two classes of activities, with such readjustment of duties as had not already been put into effect, in connection with conduct of the Laboratory's business and that of the division. The new assignments were as follows:

Allen, Deputy Chief of Division 1, was appointed by Stewart, Contracting Officer for OSRD, as his Authorized Representative with respect to the Geophysical Laboratory contract. Adams was the Authorized Representative for all other Division 1 contracts. Dr. N. L. Bowen now was appointed in Adams's place as Official Investigator for the Laboratory's contract. Bowen had been for 20 years a staff member of the Laboratory, and returned as a temporary employee on leave from the University of Chicago. Adams, who retained administrative supervision of the Laboratory, designated Greig to act in his behalf in connection with various Laboratory matters, including custody of governmental and military equipment and materials.

In the early summer of 1943 the Geophysical Laboratory's position as "central laboratory" was shared with the Franklin Institute. The latter's designation was made because of the success of the "universal" erosion testing gun developed there to facilitate the search for erosion-resistant materials. Allen had proposed the construction of such a gun shortly after he became a Member of Section A–A. Although the erosion vent plug tests at the Geophysical Laboratory were yielding interesting results in a preliminary survey of the erosion resistance of different metals and alloys, it was recognized that such a device could not give a complete answer because it did not bring into play the mechanical forces involved when a projectile is actually fired.

The only alternative at hand was to use some existing gun, such as a caliber .30 machine gun; this was being done at Johnson Automatics, Inc., in co-operation with Professor John Wulff of the Massachusetts Institute of Technology. Wulff had been interested for several years in the problem of erosion, and even before Section A–A was organized had made a suggestion to the National Inventors' Council for a possible corrective. Because the

work already started at Johnson Automatics fitted so well into the Section's program, Contract OEMsr-465 was arranged with Johnson Automatics beginning May 1, 1942, and Wulff was made a Consultant to the Section. Two months later Contract OEMsr-608 was arranged with the Massachusetts Institute of Technology so that Wulff might have facilities for the preparation of special alloys for erosion tests by Johnson Automatics and the Geophysical Laboratory.

From the experiments at Johnson Automatics, Allen recognized the much greater potentialities of liner tests as compared with vent plug tests. The caliber .30 gun, however, even though it was being fired with a heavy charge of double-base powder, did not directly represent hypervelocity conditions. Also, the liners being used in this gun were very short and not rifled. Allen proposed the design of a caliber .50 erosion-testing gun made of several parts so that rifled liners could easily be inserted for test. In order that such a gun might be constructed and tests of liners carried out, Contract OEMsr-533 was entered into with the Franklin Institute beginning June 1, 1942, for the "design, construction, and testing of erosion-resistant linings for gun barrels."

A firing range was set up in the basement of the Institute building in Philadelphia and the erosion-testing gun was built; it consisted of a 20-mm. forging bored and rifled for caliber .50, and chambered to use a 20-mm. cartridge case. By this means hypervelocity could be obtained by firing a standard-weight caliber .50 bullet with a very large charge of powder. This barrel was mounted in a 37-mm. tube so that the breech block and firing mechanism of this tube could be used, which made for convenience in operation. The work was placed under the able supervision of Dr. Nicol H. Smith, an Associate Director of the Institute. Frank R. Simpson drew upon his long engineering experience in devising solutions for the many design problems that arose.

After the caliber .50 erosion-testing gun had been in operation a short time, its versatility for erosion tests became apparent, and additional projects were added to the original group from time to time including study of: erosion at the joints of short-bore liners, effect of streamlining powder gases, effect on erosion of a protective film applied to the bore surface, relative erosiveness of propellent powders, effect of bullet and gun design on erosion, pre-engraved projectiles.

The project on pre-engraved projectiles was an outgrowth of some experiments made in an attempt to separate mechanical wear from the other factors in erosion. The very large decrease in erosion that the use of these projectiles brought about led to an intensive development of them as a practical means of reducing erosion in a hypervelocity gun, as is described more fully later.

The increased use of the .50-caliber erosion-testing gun brought the

Franklin Institute into contact with many of the other contractors of the Division. In co-operation with the Leeds and Northrup Company, measurements were made of the heat input to the bore surface of .50-caliber gun barrels in the course of the investigation of the erosiveness of different powders. The Institute co-operated with the Westinghouse Corporation by testing liners of Alloy X; with the Bell Telephone Laboratories in the testing of coatings of molybdenum deposited by the carbonyl process; with the Climax Molybdenum Company by testing liners of chromium-base alloys, and with the National Bureau of Standards in the development of improved chromium plate.

Later the group at the National Bureau of Standards interested in ballistic measurements co-operated in ballistic firings of a 37-mm. gun using pre-engraved projectiles which had been developed at the Franklin Institute.

The gun liners of special materials and the steel barrels fired with special propellants were studied at the Franklin Institute during the course of firing tests by means of gaugings and by examination with a boroscope. Once the test had been completed much valuable information could be obtained from the eroded liner or barrel by means of a metallographic examination. The Institute itself lacked facilities to make such examinations; at Wulff's suggestion a contract was arranged with Harvard University, beginning in June 1942, to make use of the metallurgical laboratory there. The work was placed in charge of John N. Hobstetter, a young instructor who developed into a keen analyst of erosion phenomena. Fired liners were sent from the Franklin Institute to Harvard, where they were dissected and examined. The reports that were promptly issued then served as a guide to those planning the next group of experiments.

# CHAPTER XXXIII

## *OPERATING THE DIVISION 1 MACHINE*

THE MONTHLY meeting of the division members became one of the division's most useful devices. The clash of mind on mind is most effective in an open meeting. Many of the division meetings were of the open type, that is, attended by invited guests; those who attended were urged to have their say.

The first section meeting was held December 3, 1941. It was an "executive session," for staff members only, as was the next, on December 16. The next three meetings, held in March, June and August of 1942, were thrown open to the Army and Navy, represented by its liaison officers, and very interesting discussions were held, often running into several hours. Of the three other meetings that year, only one was closed.

Beginning with January 1943 regular monthly meetings were held, without exception until March 1945, when two meetings were held the same day, one open and one closed session. Except for the month of June 1945, the series of monthly meetings was unbroken from January 1943 to and including the meeting on September 10, 1945. A "contract termination" meeting was held February 27, 1946, and a final meeting on June 28, 1946.

Of the 41 division meetings up to the latter date, 24 were executive sessions, 8 were open sessions, and 9 were combined affairs, that is, an executive meeting and an open one on the same day. The open sessions were well attended, invitations being regularly extended to representatives of the Army, the Navy, the British, and in some cases the contractors. The visitors often presented new thinking or information which aided materially in focusing the division's viewpoint and plans.

The committee system was found necessary in Division 1 when its multifarious activities made supervision increasingly complex. The division's extensive committee plan began with the three special Reviewing Committees appointed at the Division Meeting of July 20, 1943. These committees, consisting of one division member assisted by a Technical Aide, were to investigate and report on three major projects then underway, respectively, liners, sabot projectiles, and the automatic 20-mm. gun.

The committee on liners, for example, put the spotlight on the great obstacles that had to be overcome in this field. Of the four leading erosion-resistant materials then being considered, chromium lacked sufficient cold ductility to make a satisfactory liner; molybdenum and tungsten were not

obtainable in a size large enough for liners; and tantalum was too scarce for use except as a plating or coating on the bore surface.

The three Reviewing Committees submitted their reports at the August and September Division Meetings. Their careful reviews and practical recommendations paved the way for continuing the committee plan, and at the September 21, 1943, Division Meeting three Steering Committees were authorized, one for the Automatic Cannon, one for Sabots and Tapered Bores, one for Resistant Materials. These were intended, Adams noted, "to continue to review different phases of the Division's work, and to take an active part in guiding it." [1]

Allen accepted a roving assignment whereby he became an ex-officio member of these committees, and met with them whenever possible. Burlew, as Secretary of the division, also served on request of the committee chairmen, especially in the preparation of reports.

Each of the steering committees kept itself informed as to the progress of the work under the contracts that came within its purview. In order to make this as convenient as possible, the members and technical advisers of the committees were supplied with copies of the contractors' monthly reports. The technical adviser of each committee furnished the chairman with information as needed; in some cases, information was gained by visits to the contractors, either by the technical adviser or by a member of the committee. This active follow-up enabled each steering committee to recommend additional work requiring formal authorization under the various contracts. At intervals each steering committee presented to the Division Chief a compre-

[1] In his circular letter of September 25 to division members, Adams listed these three Steering Committees, with duties and memberships as follows:

*Automatic Cannon Committee:* E. L. Rose, aided by F. W. Cummings of the Engineering and Transition Office as Technical adviser. Contract OEMsr-746 with Johnson Automatics, Inc.

*Sabot and Tapered-Bore Committee:* Briggs, Chairman; Bleakney; Greig as technical adviser. The following four contracts were of concern to this committee, and in addition the work on sabot projectiles at the Geophysical Laboratory (Contract OEMsr-51):

(1) University of New Mexico (OEMsr-668).
(2) Arthur D. Little, Inc. (OEMsr-886).
(3) Jones and Lamson Machine Company (OEMsr-467).
(4) Bryant Chucking Grinder Company (OEMsr-534).

*Resistant Materials Committee:* Rose, Chairman; Weidlein; Schairer as technical adviser. The following eight contracts were of concern to this committee, in addition to the work on the testing of liners and erosion-resistant coatings at both the Geophysical Laboratory (OEMsr-51) and the Franklin Institute (OEMsr-553):

(1) Crane Company (OEMsr-629).
(2) Westinghouse Research Laboratories (OEMsr-915).
(3) Westinghouse Electric and Manufacturing Company (Lamp Division) (OEMsr-1205).
(4) General Electric Company (OEMsr-865).
(5) Massachusetts Institute of Technology (OEMsr-608).
(6) Harvard University — in part (OEMsr-537).
(7) Bureau of Standards — Blum (Symbol 3171).
(8) Western Electric (OEMsr-1184).

hensive review of the work of the contractors with recommended alterations.

These well organized committees were of great aid to the division in maintaining close supervision of research that was steadily increasing in complexity. As anticipated, the fundamental erosion and ballistic studies proved useful to various projects, and knowledge gained in one investigation sometimes proved of great value to entirely different projects.

As the division's projects grew in scope, the committees proved invaluable in detecting the profitable phases, in delimiting the experiments, and in keeping all the projects headed toward the final goal of helping to win the war.

Real debates were common, differences of opinion frequent. The Armed Services had their own problems and angles, and for some time continued to be pessimistic. But this pessimism began to give way to enthusiastic support, in most cases, when various projects showed usable, practical results. Differences tended to become fewer as they were well aired and basic research produced facts to replace opinions and guesses, particularly as to the causes and cure of erosion.[2]

By the spring of 1944 a heavy volume of work was controlled by the Division Office at Washington. The division had become a large operating concern, directing the work of hundreds of employees and contractors' activities. Eleven contracts had been completed; some 20 contracts were underway, in laboratories and shops located all the way from Washington, D.C. to Boston, Chicago, and Pittsburgh; two far-western contracts were being carried on, in New Mexico and in Los Angeles.

Early in 1944 J. A. TenBrook, on leave from the Philadelphia Electric Company, was assigned to Division 1 by the Engineering and Transition Office of NDRC. As engineering adviser he made a thorough study of the division's organization, and on March 29 made some valuable recommendations for improvement of its engineering and manufacturing operations. He advised assignment of an engineering aide "with initiative, resourcefulness and imagination" to supervise the technical phases of all division projects, and to co-ordinate the work of project engineers which he also recommended.

During the spring, Rose, division member, also gave much thought to the division's problems, contributing some practical suggestions for translating research into useful devices. On May 25, a special Division committee was appointed to devise solutions for various administrative problems, including that of providing more adequate supervision of the contracts. This committee consisted of Allen as Chairman, Rose, and Burlew, with TenBrook assisting in an advisory capacity.

[2] Perhaps only research directors fully realize how easily a long and elaborate series of experiments can be started, which, if not thoroughly studied and regularly reviewed, may fall far short of results justifying the time and effort.

In lieu of a committee report, Weidlein, who had replaced Allen as chairman, on July 6 outlined his personal understanding of the division's changing status. He found that the division's various investigations had advanced so far as to be in transition from research to engineering, and recommended for speedy results that Allen undertake the responsibility, as Deputy Chief, for closer supervision and co-ordination of contractors' operations, with special emphasis on engineering.[3]

A memorandum from the Division Chief, dated August 30, 1944, in announcing the prospective reorganization, stated that it was designed to achieve "more adequate supervision of projects, especially those dealing with the engineering and development of the devices by which research is being translated into actual use."

To ensure adequate attention to those ends, the new organization, which went into effect October 1, 1944, included an Engineering and Development Branch. This Branch, together with a Research Branch and an Administrative Branch, constituted an operating office supervised by Allen as Deputy Chief of the division.

With the setting up of this new group the principal office of the division was moved from Washington to Philadelphia, where quarters were obtained in the Franklin Institute building.[4] It was the function of the new office to facilitate general supervision of the contractors, and to transact most of the division's business. The Division Chief's Office remained in Washington, where Adams continued to give his full time to NDRC contacts and other matters of general policy.

In this reorganization, the three Steering Committees were replaced by seven Project-Control Committees, one for each major project. Three of these (Ballistics, Erosion, and Resistant Materials) were placed under supervision of the Research Branch. The other four (Automatic Cannon, Liners and Coatings, Pre-engraved Projectiles, and Sabot-Tapered Bores) fell to the Engineering and Development Branch.

An eighth Project-Control Committee was organized in March 1945 to take care of the hypervelocity or "A–Z" gun project which had just been set up under the joint supervision of both the Research and the Engineering and Development Branches.

These Project-Control Committees,[5] advisory in character, were designed

[3] Weidlein's analysis and recommendations, destined quickly to relieve some of the Division's worst "growing pains," will be found in Appendix 5.

[4] Allen was Secretary and Director of the Franklin Institute.

[5] *Automatic Cannon Project-Control Committee* (Engineering and Development Branch):

J. A. TenBrook, Chairman (replaced in June 1945 by W. H. Shallenberger, Acting Chairman); V. Wichum, Secretary; E. L. Rose (Division Member); H. B. Allen (ex officio).

Associated Contractor: Johnson Automatics, Inc. (OEMsr-746).

to assist the Branch Heads in co-ordinating the work under each project. Each committee, under the Branch Head as Chairman, included a Technical Aide or other assistant as secretary; a division member, and the Deputy Chief (ex officio).

Allen's memorandum announcing the original seven committees stated that contractors' representatives would be invited to the committee meetings, for the following practical reasons:

*Ballistics Project-Control Committee* (Research Branch):

J. S. Burlew, Chairman; L. E. Line, Jr., Secretary (replaced by H. L. Black in May 1945); R. Eksergian (Division Member); H. B. Allen (ex officio).

Associated Contractors: Carnegie Institution of Washington (Geophysical Laboratory); Catholic University; Franklin Institute; Leeds and Northrup Co.; National Bureau of Standards (Inductance and Capacitance Section).

*Erosion Project-Control Committee* (Research Branch):

J. S. Burlew, Chairman; L. E. Line, Jr., Secretary; C. E. MacQuigg (Division Member); H. B. Allen (ex officio).

Associated Contractors: Carnegie Institution of Washington (Geophysical Laboratory); Franklin Institute; Harvard University; Leeds and Northrup Company; Western Electric Company, Bell Telephone Laboratories (OEMsr-430).

*Liner and Coating Project-Control Committee* (Engineering and Development Branch):

J. A. TenBrook, Chairman (replaced by W. H. Shallenberger, Acting Chairman); H. L. Davis, Secretary; E. L. Rose (Division Member); H. B. Allen (ex officio).

Associated Contractors: Al-Fin Corporation; Carnegie Institution of Washington (Geophysical Laboratory); Chrome Gage Corporation; Crane Company (OEMsr-1414); Franklin Institute; A. F. Holden Company; Industrial Research Laboratories; Johnson Automatics, Inc. (OEMsr-1433); Remington Arms Company, Inc. (OEMsr-1438).

*Resistant Materials Project-Control Committee* (Research Branch):

J. S. Burlew, Chairman; J. F. Schairer, Secretary; E. R. Weidlein (Division Member); H. B. Allen (ex officio).

Associated Contractors: Carnegie Institution of Washington (Geophysical Laboratory); Climax Molybdenum Company (OEMsr-1273); Climax Molybdenum Company (OEMsr-1320); Crane Company (OEMsr-629); Franklin Institute; General Electric Company; Harvard University; Massachusetts Institute of Technology; National Bureau of Standards (Electrochemical Section); Union Carbide and Carbon Research Laboratories; Western Electric Company, Bell Telephone Laboratories (OEMsr-1184); Westinghouse Lamp Division; Westinghouse Research Laboratories; Yale University.

*Pre-Engraved Projectiles Project-Control Committee:* (Engineering and Development Branch):

J. A. TenBrook, Chairman (replaced by W. H. Shallenberger, Acting Chairman); E. Bainbridge, Secretary; R. Eksergian (Division Member); H. B. Allen (ex officio).

Associated Contractor: Franklin Institute.

*Sabot Tapered-Bore Project-Control Committee* (Engineering and Development Branch):

J. A. TenBrook, Chairman (replaced by W. H. Shallenberger, Acting Chairman); A. H. Kidder, Secretary (replaced in January 1945 by E. Bainbridge); L. J. Briggs (Division Member); H. B. Allen (ex officio).

Associated Contractors: Jones and Lamson Machine Company; Remington Arms Company, Inc. (OEMsr-1368).

*A-Z Project-Control Committee:*

J. A. TenBrook, Chairman (replaced by W. H. Shallenberger, Acting Chairman); J. S. Burlew, Vice-Chairman; W. H. Shallenberger, Secretary; E. L. Rose (Division Member); H. B. Allen (ex officio).

By having the representatives of the contractors present at its meetings, the Control Committee will have the advantage of their combined scientific talent in the analysis of the results obtained on this project and in the formulation of recommendations for further work. The discussions of common problems by the representatives of several contractors also should have a stimulating effect on their work. At some sessions an assigned topic may be discussed with several prepared talks being given to insure complete coverage of the subject.

In addition to the contractors' representatives, the Army Ordnance Department, the Navy Bureau of Ordnance, and the British Central Scientific Office regularly sent representatives to the meetings. Their participation added considerably, not only to the technical phases of the discussions, but also to the morale of all concerned.

It became evident that effective progress of so extensive a project required a system of regular reports. It would be impossible to supervise and co-ordinate the research without periodic summaries and detailed progress reports.

The security rule adopted by NDRC at the outset required that the circulation of these reports be restricted to the greatest practicable extent. Each investigator, therefore, while reporting fully on his own work, received only such information as pertained rather closely to his phase of the project. These data enabled each man to appraise the progress of his collaborators, and to appreciate his part in the program. Regular reports, although they took time which might have been spent on experimenting, were useful because they concentrated attention on the problem, outlined the difficulties and their solution, expedited the research, reduced lost motion and prevented overlapping. As each investigator's advance fitted another piece into the jigsaw puzzle, his report crystallized thought on a definite phase which had been covered, or pointed out some that remained.

It can truthfully be said, therefore, that the progress reporting system as finally developed was a vital element in the success of the gun-erosion and hypervelocity project. This research, which would normally have occupied several years at least,[6] had to be telescoped into months. Every bit of progress had to be quickly recorded and appraised, every contractor's report promptly weighed and analyzed, if the requirements of NDRC, OSRD, and the Services were to be achieved in time to be useful.

All contractors of Division 1, beginning in 1943, were required to submit monthly progress reports, usually by the 10th or the 15th of the month following the period of the report.[7] The monthly progress reports were

[6]Five to ten years normally elapse, it was estimated in 1940 by the National Research Planning Board, between conception of an idea and its final perfection for market.

[7]In accordance with the usual OSRD Contract, each contractor was required to report the progress of his studies and investigations from time to time as required by the Scien-

addressed formally to the Division Chief, and were supplied in sufficient number to meet the required distribution. Recipients regularly included the Contracting Officer (Executive Secretary of OSRD); the OSRD Liaison Office, which for a considerable period of time sent two copies of each progress report to Great Britain; Tolman, Vice-Chairman of NDRC, who in one capacity or another had special concern with the program of Division 1; the Army and Navy Liaison Officers; and co-operating contractors. Division members received for their information and guidance such reports as pertained to their committee assignments; and for somewhat over a year abstracts of all contractors' reports were sent to division members from the Division 1 Office.

As the work progressed, the Division Chief and his staff were gratified to receive responses from Army and Navy officers, arsenals, and other military establishments requesting further information or details on phases of the investigations. Such responses indicated the interest with which the data and conclusions were being received by those who would eventually be applying to standard ordnance design the basic information being obtained on an elaborate scale by Division 1.

Certain contractors were required to submit special interim reports, particularly when a laboratory investigation reached the end of a definite phase or when developmental work brought a device close to the stage of application. Other interim reports took the form of weekly letters addressed to the divisional supervisor of the project. Many contractors were also required to prepare summary reports consolidating and integrating various phases of their work. Still another class of reports included those prepared by staff members and embodying either the contractors' results, results of the staff members' own investigations, or both.

When a report was considered to have sufficiently broad interest to justify a wider distribution than was possible with the few typewritten copies normally received by the Division Office, the report was prepared and issued in mimeographed form as a formal NDRC–Division 1 report. Such reports were edited and prepared for distribution in the Technical Reports Section of the Office of the Vice-Chairman of NDRC.[8]

One of the classes of reports prepared for rather wide distribution in official circles comprised the bimonthly summaries of projects that described

tific Officer for the contract (this was usually the Division Chief). The reports were to be furnished in the quantity and form specified by the Scientific Officer, including a complete final report of the findings and conclusions.

Each contractor was required to submit to the Division Office a monthly financial statement not later than the 15th of the following month. This statement was to show the unexpended contract funds and the expenditures for the month, broken down into broad categories such as salaries and wages, equipment, and overhead.

[8]This helpful Section was in charge of the Chief Editor for Divisions 1 to 4, a position held first by Dr. Duane Roller and later by Mrs. E. G. Townes.

briefly the present status of the projects currently underway in the division. They were issued in the middle of the odd-numbered months.[9]

In addition to these formal reports the Chief of the division occasionally issued a Technical Memorandum on some topic in advance of the issuance of a formal report, for the purpose either of quickly informing a limited group of persons, or of inviting comment.

As of May 1, 1945, an Annotated Bibliography of Technical Reports and Memorandums was issued by Division 1. This report listed 69 reports put out by Division 1, including several in two parts. A slightly greater number were prepared after that date. At present writing most of these reports are classified and therefore not available for general distribution.

An outstanding achievement of OSRD–NDRC, to which Division 1 contributed its full quota, was the expansion of the field of Army-Navy-civilian working relations. The total nature of World War II made close liaison a prerequisite of success, nowhere more so than in the highly complicated business of inventing, perfecting and producing war-winning weapons.

This new technique in the old field of ordnance stemmed, of course, from the unique set-up whereby a civilian scientist, the Director of OSRD, sat in the highest military councils. For the first time in the nation's history great decisions and vast activities hinged upon — sometimes even depended upon — the devices produced by the co-ordinated efforts of this "army behind the Army" — the scientists, engineers, mathematicians, technical and laboratory aides who gave our battle forces the finest equipment in the history of warfare.

At the same time, the knowledge, skill and techniques of the Armed Services had to be called upon at every turn, as the scientist's discoveries or innovations were rapidly converted into practical devices by the designer, the engineer, and the manufacturer, with the ordnance specialist applying his final and exacting tests. Everywhere the military man called the tune — yet there were many occasions when the civilian scientists had to use their own best judgment — sometimes decide important questions laden with vital military implications, even, on occasion, to back their own decisions when they ran counter to military judgments.

Such feats of co-ordination could be achieved only by highly scientific yet essentially practical organizers. They had to know not only what their civilian cohorts were doing, where they were heading and how fast they were going, but also had to command the confidence and co-operation of the military forces. That the top NDRC–OSRD command measured up to these specifications was fully attested by the commendations of the public press when the secrecy barriers were lowered after the end of hostilities.

[9]Copies were sent to the Office of Executive Secretary for distribution.

Division 1's duty was to apply as effectively as possible the policies and plans laid down by OSRD–NDRC, which emanated, of course, from the fertile minds of those who directed the agencies concerned with war research. Their own relations with the heads of the Armed Services were so close and effective, and even congenial, that Division 1's liaison task was never difficult or onerous.

Division 1's work on patents and inventions was carried on, under Adams's direct supervision, by Lt. John B. Armentrout, USNR, previously a patent attorney, who had been assigned to the Division by the Patent Division of OSRD. Dr. Sebastian Karrer, Consultant to the division, assisted greatly with his thoughtful appraisal of patent matters; and his advice on methods of handling these matters went far toward keeping Division 1's patent affairs in good order.

Although much of the division's work dealt with highly technical ballistics, and measurements concerned with thermodynamic, chemical, and physical reactions, its basic war objective involved designs for, and production of practical guns and projectiles which would attain high velocities. This entailed the invention of new devices, and improvement of old devices. Invention, therefore, was the order of the day, as in much OSRD research.

The development of new and improved measuring apparatus also required exceptional ingenuity by investigators and laboratory assistants. In the contractors' laboratories and machine shops numbers of skillful mechanics, under the spur of winning the war, frequently applied their native American genius for invention to the innumerable materials, processes and techniques connected with the division's gun projects.

As a result, there were a great many applications for patents, numerous descriptions of new devices, and reams of correspondence relating to the general subject.

The OSRD system proved adequate in Division 1 to cover the protection of the men involved, the contractors who employed them, and the Government, which under OSRD was entitled to royalty-free use of all inventions developed in connection with the Armed Services' procurement program. Perhaps the chief complaint — direct result of war pressure — was the relative difficulty and delay in communicating or meeting with those persons in the contractors' employ who were conversant with actual inventions or improvements indicated to have been made. Applications sometimes had to be carried along for months until the right personnel had been located, full and accurate descriptions made, and the papers finally "signed, sealed and delivered."

In the event of similar activities in future defense work, it has been suggested that each contractor be required to assign one person on the technical staff to assist the usual legal officer handling patents and associated

action. So far as possible he should be fully conversant with the mechanical phases of his company's work, and have some knowledge of the legal angles as well. He should be readily accessible to those handling the patent applications.

By such methods the long delays which hold up patent papers could be eliminated and this essential work made less costly and more accurate. Facilities should be provided to enable the patent section of a division to keep abreast of all developments; sufficient personnel should be allocated to promptly take care of all applications, prepare drawings and sketches when needed, record and file the documents suitably.

Those familiar with patents realize that under the pressure of war new inventions and techniques are continually being devised, some of which may be worth tens or even hundreds of thousands of dollars in industrial life. Future claims in large amounts may hinge on such details as date of conception, precise wording or detailed description of functions, results achieved, or other items of information which OSRD continually urged should be carefully recorded and put in clear form at an early date.

A reasonable investment by the Government in providing the facilities and personnel to handle legal and patent procedure is to be considered, therefore, an essential protection of all concerned.

At first the liaison between the Geophysical Laboratory and the other contractors was carried out by members of the Laboratory staff having appointments as Consultants to Section A–A of the NDRC. When Division 1 was created no members of the Laboratory staff were appointed as Consultants, consonant with OSRD policy. Consequently, liaison with these other contractors of Division 1 was handled on a contractor-to-contractor basis, with certain members of the staff of the Laboratory being designated as Special Advisers, to facilitate the interchange of information and ideas.

In similar fashion there was effective co-operation between other Contractors of the Division. Thus, Harvard University and the Leeds and Northrup Company worked hand in hand with the Franklin Institute in an extensive program of testing propellants. For the even more far-flung program of development of resistant materials, a group of some ten contractors worked closely together, often having their representatives visit each others' laboratories for discussion and observation of tests.

These liaison arrangements became an ever more prominent part of the division's program with the setting up of the project-control committees. Attendance at the monthly committee meetings gave opportunities to the contractors' representatives to talk over common problems.

The security regulations of OSRD involved the system of "compartmentation" whereby each investigator or group of investigators was informed only of those parts of the work of other groups bearing on their

problems in hand. This system included also the Division Chief, who was informed of the work done by other divisions of NDRC only to the extent to which there was an overlap of interest. Arrangements for liaison between Divisions had to be cleared through the Office of the Chairman NDRC. Division 1 had permanent liaison arrangements with four other divisions, namely Divisions 2, 3, 8, and 18.

Among the problems investigated by Division 2 was that of the effects of hypervelocity projectiles against armor plate. Divisions 1 and 2 co-operated in a number of different ways. Thus Division 2 carried out certain armor-piercing tests in order to answer questions raised by Division 1 about the effects of velocity on armor penetration. Division 1 contributed to Division 2 some of its designs for sabot projectiles, which proved especially advantageous for experimental firing against armor plate. When Division 1 felt that its contractors had developed a ballistically satisfactory projectile for the tapered-bore gun, it called upon Division 2 to help improve its armor-piercing characteristics.

Division 1 also assisted Division 2 in connection with ballistic firings. Since Division 2 was studying the problem of muzzle blast, Division 1's erosion-testing gun at the Franklin Institute and the 3-inch gun at Carderock provided information that was of value to Division 2's designers.

Close co-operation between these two divisions was facilitated by deliberately appointing Bleakney, Deputy Chief of Division 2, as a Member of Division 1.

The relations between Divisions 1 and 3 centered about common problems in interior ballistics. The work of the group at the Geophysical Laboratory on an improved system of interior ballistics was directly applicable to the ballistics of rockets, which were being investigated by Division 3, Section H. For a while Division 1 ballisticians carried out calculations for Section H. Later some members of this group were transferred to a Section H contractor. Similarly the contract with Duke University for studies of the burning rates of propellants, which was initiated by Division 1, turned out to be of paramount importance to Section H of Division 3, and eventually supervision of the contract was transferred to the latter agency.

One of the important ways of minimizing the erosion of a gun is by use of a non-erosive propellant. The development of such propellants was one of the problems of Division 8. Starting early in 1942 there was a sharing of information on this subject between the two groups. Part of Division 8's program included the development of new propellants. Samples of these, made by the du Pont Company under a Division 8 contract, were supplied to the Franklin Institute and the Geophysical Laboratory for tests of erosiveness. Dr. Wendell F. Jackson, Assistant Director of the Burnside Laboratory of the du Pont Company, serving as Consultant to Division 1, was of great assistance in choosing the proper granulations of these propellants

for test. The report of the test of one of these new propellants was cited by the Army Ordnance Department as one of the reasons for the adoption of one of these propellants as standard.

One of the many metallurgical problems investigated by Division 18 was that of resistant materials for use as turbine blades. The knowledge gained by its contractors was made freely available to Division 1 representatives in its early days of planning a program for the development of erosion-resistant materials. As some of the conditions of use of the turbine blades are similar to those existing in gun barrels, some very useful leads were obtained from those data. Close co-operation between the two divisions was continued, with H. C. Cross acting in a liaison capacity as a representative of the Chief of Division 18. The contract that Division 1 made with the Climax Molybdenum Company was an outgrowth of the work done by that company for Division 18 under an earlier contract.

On another occasion Division 1 contributed to Division 18 information about erosion-resistant materials in general, as a guide for planning an effective means of minimizing the erosion of rocket throats. Subsequently two of Division 1's contractors, the Westinghouse Lamp Division, and the Western Electric Company, furnished samples of erosion-resistant materials to Division 18 for tests in rocket throats.

# CHAPTER XXXIV

# *MEANS TO REDUCE EROSION*

IN ALL Division 1 developed six practicable methods for attaining hypervelocity. Some of these sought to quell erosion as an end in itself and as a stepping-stone to the end result; they are the subject of this chapter. Others aimed more directly at the main target; they are the subject of the next chapter.[1]

## Gun Liners

Section A–A's ambition to develop a practical gun liner was stimulated during the June 18, 1942, section meeting at Washington. Hypervelocity was the major subject for discussion along with various proposed means for attaining it, such as the sabot projectile and the tapered-bore gun.

But these, it was pointed out, would take time to perfect — and something useful was wanted at once! Rose urged a short replaceable steel liner as a possible early solution of the erosion problem, applicable to various sizes of guns. This would require making the gun barrel with a bored-out recess at the breech end, into which the interchangeable, replaceable steel liners could be inserted by a suitable method.

Full-length, shrunk-in liners are commonly used in guns of 5-inch caliber and larger. Our Ordnance Department had for a number of years used 3-inch guns with full-length "loose" liners, and some of these guns had been relined several times, showing that the life of the standard gun barrel is far longer than that of the bore surface. The idea of a *short* liner, however, was not generally considered practical, and no work was underway along that line.

Short liners tried by the British in World War I had shown severe defects. Insertion difficulties were considerable. Section A–A's people felt confident that the insertion technique could be made practicable, and that a replaceable liner, even if made of ordinary gun steel, would be at least a partial solution of the erosion problem.

While research was perfecting the technique of steel liner design and insertion, other parallel research could perhaps be selecting or developing new materials which later could be inserted as erosion-resistant liners. In accordance with this general plan the Crane Company was assigned a

[1]Unusual space is given to the projects dealing with liners in this chapter and sabot projectiles in the next chapter, first, because of the exceptional difficulties that were surmounted; second, because of the lessons in co-operative research that were learned.

really tough contract[2] for designing (a) a practical steel liner, and (b) a usable method of inserting a series of such liners in a medium-sized gun. The 75-mm. gun was selected as "guinea pig," because one was readily obtainable.

The Crane Company made a valiant effort at a quick solution of its vital war assignment. But its top-grade technicians and mechanics soon discovered some of the excellent reasons why neither the Army, the Navy nor the British were putting "field-removable" steel liners in their guns. After some months of earnest efforts, they were about ready to say it couldn't be done! That is, a practical liner, suitable for use in the field, seemed almost unattainable by standard mass-production methods.

Clearances between the outside of the liner and the inside of the gun-tube had to be *large* enough so that the liner could be forced in; at the same time, tolerances had to be *small* enough so that the liner would "stay put" without rotation, forward movement or serious expansion, even after heavy firing. Unless this were done, performance would suffer and the gun barrel, bored out to make a recess for the liner, might not be strong enough to withstand the powder pressure and the shocks of firing.

The British inadvertently introduced a further complication for us. They had had trouble with liners turning in the barrel during firing; such turning affected the accuracy of the gun. Crane Company designed a liner of elliptical cross-section, so that once in place it could not shift its position. The elliptical form caused complications in both design and production, and was later shown to be unnecessary. For this and other reasons little net progress was to be seen after six months of effort.

Meanwhile, the Geophysical Laboratory was pushing erosion resistance hard, and had set up a caliber .50 machine gun for the purpose of testing the suitability of materials for use as a liner. The materials were inserted as liners or as bore-surface coatings on steel liners. Successful methods of inserting liners for test purposes were achieved but none of these designs was, or was intended to be, suitable for mass production.

A great variety of metals and alloys — more than a hundred materials in all — were evaluated in erosion vent plug and other laboratory tests, and the more promising were tested as liners or as coatings on steel liners, at the Geophysical Laboratory, at the Crane Company and at Franklin Institute. At the latter the hypervelocity caliber .50 erosion-testing gun described earlier proved very effective in quickly determining the potential value of materials for gun barrels or liners.

One thing was definitely learned from these tests: because of chemical and thermal attack by hot powder gases, not even the finest of high-speed steels, die steels or gun steels, or any ferrous alloy (having iron as a leading component) could possibly be good enough to resist severe erosion very

[2]OEMsr-629, awarded June 15, 1942.

long. Not over 25 per cent improvement could be expected from any ferrous alloy. That would certainly not do under hypervelocity firing.

In machine-gun tests, barrels with liners were fired against a paper target, perhaps for several hundred rounds. Then, some elongated holes would appear among the clean round ones; the former were produced by bullets which had not traveled straight to the target. When a given proportion of shots produced this "keyholing" the accuracy-life of the lined barrel was considered to be ended.

Or, the bullets would begin to "disperse," the bullet holes slowly but surely widening the pattern as the liner and barrel from which they came began to exhibit wear. Worn rifling and eroded lands were not the only causes of this dispersion, which is one of the chief criteria of a gun's life. Measurements and analysis revealed that a certain degree of expansion of a hot machine-gun barrel, after it had fired long bursts, resulted in failure of the bullets to engage the lands and grooves, with consequent tumbling in flight.

By November 1, 1942, nine contractors were at work on half a dozen phases of resistant-material development; another joined the group the following June. On liners and coatings the Crane Company and Franklin Institute were hard at work by June 1942; they were joined in the following two and a half years by five other contractors. In all, before the work came to an end, nineteen research and manufacturing concerns had joined their resources to solve the problems of the three interrelated projects of resistant materials, liners and coatings, and erosion.

The Crane Company's "liner experts" eventually were able to specify a practical way of putting a steel liner into a medium-sized gun, making it stay there satisfactorily, and replacing it when worn out. This was real achievement — certainly basic to the use of replaceable liners in gun barrels. Furthermore, even though the replaceable steel liner was not adopted by the Army during the war as a practical military measure, this liner enterprise was found to advance the entire technique of designing and inserting gun liners.

The fundamental studies on causes and cure of erosion were now getting results: physicists and chemists were beginning to know the how, why, and wherefore. Burlew later summarized (at a division meeting in November 1944) the vital role that these physicochemical studies had played in the advance of the erosion-control forces. He stated:

> Even after two years of patient investigation we still did not have a complete understanding of the process of gun erosion; but in that time we had accumulated enough knowledge so that by the spring of 1943, when the experience of aerial combat indicated that erosion was limiting the performance of the caliber .50 aircraft machine gun, we were in a position to set to work to improve that weapon. The solution of the problem that has been found, namely the use of a

short breech line of Stellite, is a solution that we might have stumbled on; but the exploitation of even a lead in this direction has been carried out more expeditiously and with greater certainty because of insight concerning the factors involved in the deterioration of machine-gun barrels.

To make a successful bore-surface material, the division's investigators had learned, a material must have high "impact strength," good ductility and workability, rather high melting temperature, and chemical inertness. In addition, it must be available in quantity. The best materials, as to erosion resistance, always seemed to be the scarcest.

Stellites had been considered at the beginning of Section A–A's project. Greig had a conversation with Dr. C. W. Drury of the Department of Munitions and Supply in Canada, in which the latter had emphasized the importance of hot-hardness in dealing with the gun-erosion problem, and had advocated the investigation of cobalt alloys. Adams, to whom Greig reported this suggestion, discussed the subject with MacQuigg, who was then a member of the Metallurgical Section in Division B, and later with Drury himself. Subsequently, in order to test the value of high hot-hardness, high-speed tool steel and a Stellite alloy were tested as erosion vent plugs and showed high weight losses. The poor resistance of Stellite to flash melting in this test masked its resistance to chemical attack by powder gases.

In order to evaluate the relative importance of hot-hardness and chemical resistivity in controlling performance of bore-surface materials in a rapid-fire gun, a reconnaissance series of different materials of known hot-hardness was selected for firing tests at Crane Company; as short breech-liners in the caliber .50 heavy machine-gun barrel, liners of 18-4-1 high-speed steel, two hot-die steels and a Stellite were tested on a severe firing schedule. The results on the first three materials were very disappointing. Their hot-hardness could not be utilized because of severe powder-gas erosion. The Stellite liner, on the other hand, was found to be resistant both to thermochemical attack by powder gases and to deformation of the rifling.

Since a test of materials of outstanding hot-hardness was desired the particular Stellite alloy chosen for the test had been selected because it was the hardest commercial grade of Stellite that could be rifled with carbide-tipped cutting tools. In the firing test the Stellite liner had shown little or no powder-gas erosion, deformation or wear, and its performance was of a different order of magnitude than gun steel. However, the liner had cracked and such a liner would be unsafe for Service use. Hot-hardness had been overemphasized at the expense of ductility; moreover, since the liner had moved forward during firing, the insertion problem remained to be solved.

The insertion problem was solved at Crane Company by the use of a flanged type of liner design, and the problem of dangerous failure by

cracking was solved by the selection of a different Stellite alloy composition with slightly less hardness but much greater ductility. Liners of this alloy were prepared at Haynes Stellite Company[3] as investment castings. These were inserted in caliber .50 machine-gun barrels and tested.

The first of the new liners completed for a caliber .50 heavy machine-gun barrel was tested under a severe schedule of 500-round groups, made up of five 100-round bursts with one-minute cooling between bursts. To the amazement of even the most optimistic, this history-making liner, after a long series of firings in the firing range at Crane Company, was still in usable condition when the steel barrel was completely worn out. During July 1944, somewhat longer liners, placed in the lighter-weight caliber .50 aircraft machine-gun barrel and necessarily fired on a different schedule, withstood extensive firing with but slight wear; the rifling was still sharp, with no flattening of the lands. Careful inspection of the bore surface with an illuminated "bore searcher" showed it to be in good condition, with no cracking, further proof of Stellite's ductility.

News of this success quickly spread, but the Division staff could not take the time to make a formal report. At the request of Bush the development was summarized by Adams in a memorandum dated August 5. Adams pointed out that the Stellite liner was an outgrowth of extensive investigations of gun erosion; in large and medium-sized guns with high muzzle velocities, these had produced good evidence of actual melting of a thin surface film. This melting appeared to be the dominant factor in ending the gun's useful life.

But in the machine gun, particularly at high temperatures produced by prolonged firing, the dominant factor was observed to be the ironing out of the lands as they became softened. This had been the motivation for testing every kind of material that had the ability to retain its hardness at elevated temperatures. Oddly enough, that did not necessarily mean a material with high melting point! In conclusion, Adams stated:

> While Stellite appears to be an answer to the machine-gun problem, there is no reason to expect that it will show much advantage in larger guns. . . . Stellite has been shown to provide a significant and worth-while improvement in the performance of the caliber .50 aircraft machine gun by permitting that gun to be fired in longer bursts and at more frequent intervals without destroying the barrel. During such firing the velocity remains constant instead of decreasing steadily as it does with a steel barrel. Furthermore, even if the burst is sustained so long that the barrel loses its accuracy because of expansion, accuracy is restored simply by cooling, whereas a steel barrel fired under such conditions would become useless. The value of this development obviously should be measured in terms of both the increased effectiveness in offense and the increased insurance against loss of crew and plane.

[3]Contract OEMsr-1330 with Union Carbide and Carbon Research Laboratories.

Even before the Stellite liner was fully perfected, the division felt so confident on the basis of tests underway that it decided to freeze the design and proceed at once with the production of 200 test barrels, by modifying standard steel barrels through the insertion of Stellite liners.

The 200 barrels went to various Ordnance Department testing centers, and withstood the tests to the complete satisfaction of the Armed Forces. Therefore the Crane Company was requested by NDRC to produce 2000 of these same barrels for extensive Service tests in actual combat.

The new contract, awarded the Crane Company for this purpose, also afforded an opportunity to develop pilot-plant methods prior to production on a large scale. The initial development of Stellite had been so well done by the Company's research division, under John P. Magos, that the pilot production was readily handled. Because F. S. Badger, Vice-President of the Haynes Stellite Company, kept production on schedule, a plentiful supply of Stellite liners was always available. The 2000 barrels were turned out in about six weeks after initial production was started. Many of these barrels went overseas, some as far as India and Burma. Some were flown directly to areas of combat for testing under actual fighting conditions.

Just when the division felt itself about to cross the threshold of success in its liner project, the Resistant Materials Project-Control Committee had to report that experimental firings had revealed unexplained and undesirable dimensional changes in some Stellite liners.

Prompt measures were taken not only as to those difficulties, but also to initiate more intensive studies of Stellite as a liner material. A Stellite Advisory Committee was appointed, headed by Burlew, with Schairer assisting. The contractors concerned were represented as follows: P. H. Brace, from Westinghouse Research Laboratories; W. A. Wissler from Union Carbide and Carbon Research Laboratories; N. A. Ziegler from the Crane Company.

The committee was told that its main objective was to eliminate any "anomalies" which might be caused by dimensional changes during firing, and might even be affecting Stellite's erosion-resisting qualities. Some experimental liners showed constriction of the bore during firing. Two apparently identical liners gave different results when fired. Also, there were unexplained variations in hardness.

The committee was further told to look into other methods of casting, on the theory that investment-casting, in use up to that time, might not be the best method. Other contractors could be called in, laboratories utilized for special work such as X-ray and electron-diffraction measurements, metallographic studies, hardness measurements, and so on. Fabricating, machining, rifling and other production steps were to be investigated fully.

The final report of the Stellite Advisory Committee was made as of Octo-

ber 5, 1945; summarizing the chief accomplishments of the contractors of the Division in this field, it ended as follows:

> The committee feels . . . that Stellite is an alloy composition that happens to have a nearly unique combination of properties that makes it suitable for use as a machine-gun liner fired with a single-base powder.

"In view of these findings," Allen wrote December 6, 1945, to Adams, "I think that the committee has satisfactorily performed the task assigned to it and that Division 1 in turn, through this report has fulfilled its obligation to furnish the Armed Services with grounds for confidence in the reliability of Stellite as a gun liner material."

The test data proved that the new combination barrel withstood 10–15 times the number of rounds that would wear out the standard caliber .50 barrel when fired on the same schedule.

Concluding this memorandum, Adams proposed that 100 caliber .50 aircraft machine-gun barrels be manufactured as above described.

The Ordnance Department tested caliber .60 machine-gun barrels with Stellite liners supplied by Division 1 for test. This hypervelocity gun when fired with single-base propellant had a muzzle velocity of 3500 feet per second. On the same firing schedule the life of the lined barrels was several times that of a standard gun-steel barrel.

One of the most satisfying demonstrations of Stellite was the firing of liner-equipped caliber .50 aircraft barrels continuously for hundreds of rounds, to the point of complete keyholing (all bullets "tumbling" from their true trajectory because of failure to engage the rifling); when the barrel was cooled, and fired again, its performance proved to be practically as good as ever! Old-time ordnance specialists and gunners had to see this to believe it, but were thoroughly convinced when such demonstrations were repeated at will. They were even more astonished to see the Stellite liners fired at temperatures above red heat — without serious loss of accuracy or physical properties.[4]

Early in 1945 the Ordnance Department had undertaken large-scale production for Service use. The division assisted the Ordnance Department's Machine Gun Integration Committee in putting a half-dozen manufacturers to work making Stellite-lined caliber .50 barrels.[5] Production by the Ordnance Department mounted steadily each month of the first half of 1945. Even so, the superiority of the Stellite-lined barrels was such that urgent demands for quick deliveries piled up, and priorities had to be established.

[4]However, the division knew that the relatively low melting point of Stellite makes it vulnerable under the high temperatures of double-base powders. For use with such powders an entirely new material was required, such as the hardened "Alloy X" likewise developed under division auspices, and described in a later section.

[5]See "The Industry Ordnance Team," Lt. Gen. Levin H. Campbell, Jr. Whittlesey House, New York 1946, p. 247.

## ALLOY X

At a staff meeting of the Geophysical Laboratory on July 23, 1942, the tests which had been conducted during the previous six months on erosion-resistant materials were reviewed. Adams suggested that the whole field of alloys be explored, and that the liner research, now narrowed down to three or four of the most promising materials, be devoted to developing these materials in a form suitable for final test as gun liners.

Adams also recalled that "because high velocity increases erosion so much, it may be necessary to cut out 95 per cent of the erosion, and in order to do so a material not suited to use as a whole gun might have to be used as a liner."

The properties of Alloy X are so remarkable as to justify concealing even the basic metal from which it was evolved. This basic metal had been tested some time previously by one of the Services, which, however, was not at the time actively seeking an erosion-resistant gun-barrel material. Nor did Division 1 realize, when first studying the record of these tests, the full implications of this material.

Not until some months later, after numbers of vent-plug and preliminary firing tests had been made, was it realized that this material in a gun barrel showed practically no chemical erosion, heat checking or cracking — even after severe firing. At that time, however, it was too soft for liner use, as shown by deformation of the rifling after firing.

By September 1, 1943, three contractors were actively engaged in different phases of the development, producing numerous small specimens of alloys containing varying percentages of the basic material. It was vital to overcome rapidly what the Liner Committee described as a "complete lack of information on the working properties of its alloys."

Not the least of the many troubles at this time was the delay in obtaining suitable pieces in sizes and shapes for test purposes. Then, too, the insertion of liners made from special erosion-resistant alloys was proving something of a mechanical problem.

The September 15, 1943, Division Meeting was heartened by the report that certain preliminary experiments had been "unexpectedly successful" with a workable alloy of this material. On January 15, 1944, the Resistant Materials Committee declared, in requesting additional funds for an all-out effort by the contractors:

> Alloy X shows very superior resistance to erosion by the powder gases and should be suitable in this respect for a bore surface even under the most severe conditions in a super-gun of high muzzle velocity. It remains to be seen whether Alloy X can be hardened sufficiently to resist the swaging action on the lands and the wear of the projectile, especially in a rapid-fire gun.

The same committee reported on June 1, 1944, that the division's intensive efforts to produce an "ideal" erosion-resistant material were getting results. These efforts were being concentrated on the production of hard alloys which would be machinable and workable. Firing tests under hypervelocity conditions in a small-caliber weapon showed remarkable performance and indicated the eventual suitability of the material.

"It appears now," said the committee, "that new emphasis should be given this part of the program, and that a project for its ultimate application to the hypervelocity gun should be initiated at an early date."

In October 1944 equipment was being assembled for preparing large ingots — a tremendous step from the status of a year before. Processes had been devised for producing crack-free ingots. Hardening and working methods to increase strength and ductility were steadily improving. The division had been aiming at an alloy which would stand up under the high temperatures produced by hypervelocity firing with double-base powder.

It was not until its meeting of May 17, 1945, that the Resistant Materials Committee was able to report that its contractors were regularly producing a high-melting alloy of high strength, moderate ductility and hot-hardness comparable to Stellite. A representative of one of the contractors was recorded as declaring during this meeting that the machining and other processing operations had been so improved that gouging and tearing of the metal had been eliminated; in fact, that the problem of machining Alloy X had been satisfactorily solved.

At the Division Meeting of July 13, 1945, it was announced that the Navy Department had requested an extension of Project NO-23 to cover development and manufacture of erosion-resistant liners. The project, accepted by NDRC, was assigned to Division 1.

This provided the necessary support for attempts at fabrication of large size Alloy X liners by the Westinghouse Lamp Division. The experience and wisdom of the group under Dr. J. W. Marden, the contractor's director of this project, had enabled Division 1 to overcome the difficulties of hardening and inserting liners in a small-caliber weapon; it now was possible to proceed with the fabrication of Alloy X liners for medium-caliber hypervelocity guns. Close co-operation was maintained with a group at the Westinghouse Research Laboratories under P. H. Brace, Consulting Metallurgist.

Although it was not possible to complete this project before the end of the war, the Navy Department proved its deep interest by taking over the project as part of the contract termination program. In announcing this intention under date of August 30, 1945, the Navy Bureau of Ordnance commended the Division's work in these words:

> It is considered that the work done by Division One in the development and application of erosion-resistant materials for gun bores has great promise in the

field of high velocity guns. The Bureau of Ordnance is vitally interested in the application of short . . . liners to high velocity guns. It is urged that the NDRC continue its work on . . . liners as long as its policies permit, and when termination of NDRC participation in the work becomes necessary, the Bureau of Ordnance will undertake its continuation.

## Chromium Plating of Gun Bores

Division 1's investigation into the plating of chromium on gun bores began in 1941, when some isolated experimental data were uncovered. It was learned that one of the first applications of chromium plating to the bore surface of a gun (caliber .30) began about 1926 at Frankford Arsenal in Philadelphia.

In 1928 the Washington Navy Yard applied chromium plate to the bores and outside surfaces of some large Navy gun tubes. The plating proved so useful in preventing salt-water corrosion that all Navy gun tubes thereafter, from 1-inch bore upward, were plated there.

Chromium plating of caliber .30 rifle barrels was being done at Springfield Armory in the summer of 1941. Adhesion was satisfactory under the mild firing schedules of that day. Watertown Arsenal began some plating work about the time Division 1 entered the picture.

Division 1's active interest in chromium was initiated as a result of studies under the microscope of sections of a worn chromium-plated gun. These studies, made by Merwin at the Geophysical Laboratory, revealed that while the inner bore surface had generally been eroded away, bits of the original chromium plate still embedded in the steel retained their clean sharp surfaces. In other words, the hot powder gases had caused no alteration of the chromium particles. Dr. P. R. Kosting of Watertown Arsenal had previously observed that chromium is much more resistant to the chemical effects of powder gases than is steel.

Tests of plated steel liners in machine-gun barrels showed that adhesion was uncertain, and had to be much improved for the hypervelocity guns envisaged by Division 1. The technique of plating required extensive experimentation until it was possible to achieve process control and reproducibility. Not only could a given thickness of plate eventually be deposited at will, but the exact hardness of plate could be controlled.

Alloys of chromium, as well as pure chromium, were experimented with during 1942 and thereafter. A large number of barrels plated by the Electrochemical Section of the National Bureau of Standards, under the direction of Dr. W. Blum, were given firing tests by the Geophysical Laboratory. Over 100 types of plate were tested during the evolution of a plate that would adhere to the surface even under the severest firing schedule, and at the same time resist the wear and abrasion of the projectile. Part

of this investigation was concerned with the effects of the plating conditions on the composition, structure and physical properties, including hardness, of the chromium deposits. Contraction of the plate after it was heated by firing, and the development of an extensive crack pattern, proved to be a troublesome factor.

Blum, in a post-war summary of the National Bureau of Standards' work, noted that reports from similar work in England had been received; and methods and results had been discussed with various English visitors. He remarked, incidentally, on the cordial co-operation between the two countries; however, he noted, "progress might well have been accelerated if there had been more exchange of visits of those directly engaged in the plating, firing and examination of the guns."

The Resistant Materials Committee as of June 1, 1944, reported another line of experiments which had begun to produce results. Firing tests at the Geophysical Laboratory had shown the very favorable effect produced by the nitriding (surface hardening by use of ammonia) of ordinary gun-steel barrels, prior to chromium plating. The purpose of nitriding was to harden the surface so as to prevent flattening of the lands under the impacts of the projectiles during firing, and at the same time to give adequate support for the plating.

When aircraft machine-gun barrels were thus protected against engraving stresses by nitriding, and against thermochemical attack of the powder gases by chromium plating, they gave very much improved performance. The nitriding of steel machine-gun barrels was carried on for Division 1 by commercial firms.

It was found that nitriding alone gave little if any added resistance to powder-gas erosion. Only when the nitride-hardened steel bore surface was protected from powder-gas erosion by an erosion-resistant layer of chromium plate could its hardness be utilized to prevent deformation on the rifling by the swaging impact of the bullets. The combination did the trick. Once the effects of the thermochemical and mechanical factors in erosion had been pretty well separated by Division 1's investigators, the combination of chromium plate and nitriding was an obvious means of combating them, although one that required considerable extension of technique to make practical.

An essential feature of the chromium plating was a "muzzle choke," obtained by carefully thickening the plate toward the muzzle, greatly increasing the accuracy-life as compared to that of a standard barrel. Thus the steel caliber .50 aircraft machine-gun barrel was given a remarkably long accuracy-life — far higher than when unplated — even slightly better than that of the same barrel with a Stellite liner. This held true for any of the usual firing schedules. Numerous firing tests clearly proved that the muzzle

velocity of plated barrels was initially from 50 to 100 feet per second higher than that of unplated barrels, but decreased at an earlier stage of its life, as compared to Stellite-lined barrels.

By November 1944, the commercial application of the graduated chromium plate to nitride-hardened steel bores of machine-gun barrels was required. This was the pay-off in Division 1's second short-range project for improving machine-gun barrels. Success with erosion-resistant plate in hypervelocity guns was not assured, but looked very promising. Meantime, here was a method for making over the enormous numbers of caliber .50 aircraft machine-gun barrels then on hand, in order to improve their combat performance. Fortunately, the nitriding-plating process was cheap and easily applied to all new barrels as they were turned out by manufacturers, as well as to the modification of the large stock of standard steel barrels on hand.

Events now moved swiftly. Adams proposed that the new process be applied to many of the barrels then in depots or warehouses, to provide quickly and easily a large supply of superior barrels. The Ordnance Department, which estimated that there were over a million barrels available for chromium plating, was anxious to learn of available facilities so that plating could start at once. Blum estimated that 3000 barrels could be plated daily by existing chromium-plating facilities.

In order to expedite the commercial development of the process for modifying standard steel barrels or manufacturing new barrels with graduated chromium plate on hardened bores, Division 1 established a pilot plant by contract with the Chrome Gage Corporation of Philadelphia. This pilot plant studied the problems of large-scale manufacture and the controls and techniques necessary to insure the quality of barrels in mass production.

The performance of the Stellite-lined barrel was compared with that of a nitride-hardened barrel chromium-plated to give a constricted bore toward the muzzle, and reported by Dr. E. F. Osborn of the Geophysical Laboratory in the following words:

> Both types are far superior to the standard aircraft barrel now in use. Both types are essentially the same weight as the present barrel and are identical in outside dimensions. . . .
>
> The Stellite barrel is notable for its long velocity-life and the nitride-plated barrel for its long accuracy-life. Consequently, if, for a particular type of mission, maintenance of muzzle velocity is of great importance and the firing schedule is not likely to be sufficiently severe to cause the barrel to fire rounds which yaw, the Stellite-lined barrel would be preferred.
>
> If, on the other hand, accuracy-life is of most importance and some muzzle velocity drop can be tolerated, or if a very severe schedule is to be fired, the nitride-plated barrel would be more suitable. In other words, for ground strafing,

using long bursts, I would recommend the nitride-plated barrel. For air combat I would think the Stellite-lined barrel would be preferred.

When both modifications were applied to the same caliber .50 aircraft machine-gun barrel by chromium plating (with choked muzzle) the steel bore ahead of a Stellite liner, even more outstanding performance was achieved. This combination, except for the choked-muzzle effect, had been conceived even before the separate features had been made successful; it was developed by the Geophysical Laboratory and Crane Company in co-operation.

Numerous firing tests of such barrels by these two contractors showed clearly that graduated chromium plate ahead of a Stellite liner nearly doubles the effective accuracy-life as compared with a barrel with the same liner but no plate, except when fired a single long burst, in which case the increase is about 20 per cent. The test data proved that the new combination barrel withstood 10–15 times the number of rounds that would wear out the standard caliber .50 barrel when fired on the same schedule.

Adams therefore proposed that 100 caliber .50 aircraft machine-gun barrels be manufactured as above described, for test by the Small Arms Development Division of the Research and Development Service, Ordnance Department. This was done.

Tests by Division 1 and the Ordnance Department showed that this combination of a Stellite liner and graduated chromium plate ahead of the liner produced a barrel which could be fired so severely without loss of accuracy or serious impairment of velocity, that the barrel grew hotter and hotter, and failure finally occurred when the softened barrel drooped, and the bullets tore through its white-hot walls!

Research at the Crane Company and at Geophysical Laboratory showed that changes in the weight and contour (distribution of barrel steel) of the caliber .50 aircraft machine-gun barrel to provide reinforcement in the weakest places, and the use of barrel steel with better high-temperature properties than regular gun steel, permitted the most effective use of the combination of a Stellite liner with graduated chromium plate ahead of the liner. This produced a "super-barrel" which was entirely safe and showed a life of about 30 times the number of rounds (on the same severe firing schedule) which wore out a standard steel barrel. Only V–J Day and termination of the program prevented these super-barrels from getting into production and combat use.

## The Fisa Protector

The Fisa Protector[6] is a device developed by the Franklin Institute as a Division 1 contractor. It consists of a thin metal sleeve slipped over the projectile and the neck of the cartridge case, before the round is loaded in

[6]Name derived from initials of the following: Franklin Institute, Section A.

the gun. On firing, the band of the projectile moves over the inner surface of the sleeve, and the rifling of the gun is engraved on it as well as on the band.

The sleeve acts to protect the origin of rifling from the powder gases, and also takes up the most intense stresses of engraving, thus greatly reducing abrasion and plastic deformation of the gun's lands. Most of the rifling is saved from severe engraving stress, and, when plated, from erosion of the powder gases.

At one time the Resistant Materials Committee reported that satisfactory Fisa-type ammunition for smaller calibers appeared close to attainment, and that preliminary results with 37-mm. ammunition indicated that the design might be applicable in all guns using fixed ammunition. The Committee pointed out further that Fisa ammunition does not necessarily involve any drastic modification of existing ammunition, the sleeves being added to standard rounds already assembled.

The Franklin Institute found its protector even more useful in a chromium-plated barrel. The device protected the plate from abrasion at the origin of rifling, where the plate usually fails first. Plans were underway to apply the protector to the new caliber .60 machine gun. Erosion tests were to be made in a 37-mm. gun after it had been rechambered at Watervliet Arsenal, to increase the bore diameter by the thickness of the Fisa sleeve, and the grooves deepened, to permit use of the protector.

The device was tested in caliber .50 and caliber .60 barrels, and in the experimental 37-mm. gun. In its final report of November 19, 1945, the Franklin Institute stated that its experience showed the Fisa Protector could not be used with the present 37-mm. ammunition because of the excessive interference between band and groove diameters. For satisfactory functioning the Institute recommended that the band diameters be reduced, or certain other changes be made in the ammunition.

The division considers that when perfected the Fisa Protector should be a useful adjunct to chromium plate as a means of protecting the bore surface of medium-caliber guns from erosion, by preventing cracking of the plate in the region of the origin of rifling.

# CHAPTER XXXV

# *THE QUEST FOR HYPERVELOCITY*

THE METHODS described in Chapter XXXIV were primarily devised to increase the life of a gun firing at high velocity and in long bursts or at high rates of firing. Each of them might readily enough contribute to the production of a new hypervelocity gun; but they had collateral advantages even if the goal of hypervelocity was not attained.

The division sponsored two other projects, however, which were aimed directly at the production of hypervelocity. Neither method was new; neither was currently popular; in both cases the work of the division demonstrated that the original proponents of the ideas had been far from foolish. The basic principle of both devices was to discharge from the muzzle of the gun a projectile smaller than the bore. One achieved this result by providing discardable parts (sabot); the other provided a tapered bore with a deformable projectile, changing its diameter with the diminishing diameter of the gun barrel. In either case the powder charge was able to produce higher velocity since it was expended upon a smaller diameter and a lighter projectile.

## THE SABOT PROJECTILE

"Sabot" is French for "wooden shoe," and in an ordnance context means the part used to fill the space between a small projectile and a larger gun bore; it is made detachable and is to be dropped as the projectile leaves the muzzle. Such devices were used as early as 1848 in order to adapt special projectiles for use in available guns; they were generally made of wood (hence sabot). In World War I the French used sabots to adapt 37-mm. ammunition for use in the 75-mm. gun. This gun had a low rifling pitch and the light projectiles were unstable in flight and none too accurate. American ordnance experts, mindful of this experience, and despite awareness of a reviving interest abroad, were not very interested in the sabot projectile as practical ammunition. At the time when Division 1 took up the cudgels the sabot had a bad name in American military circles and the division and its contractors therefore faced an uphill fight against opposition which was not entirely made up of the technical difficulties inherent in the problem.

Compared with tapered-bore projectiles those which employ sabots have two disadvantages. The discarded parts may scatter at the muzzle and be

dangerous to friendly personnel in front of the gun; the discarded parts have absorbed a substantial portion of the total energy and hence do not leave all of it in the projectile for application to the target.

On the other hand the sabot projectile has some advantages as well. It can be used in a standard gun interchangeably with standard ammunition without special precautions. (An adapter can be placed in a standard gun to provide a tapered bore but this has the hazard that if it should not be removed and if then standard ammunition should be used the results would be disastrous.)

The active interest of Division 1 in developing a sabot projectile was aroused by letters from Bush and Tolman to Adams on the 23rd of March, 1942. The Divisional staff was of course already familiar with the early history of the sabot but now it began to study its potentialities in earnest. Could a prerifled plastic sabot be used? The Bureau of Standards was consulted about this. What was the minimum spin required for stability? Dr. C. L. Critchfield, mathematical physicist from Harvard University, assisted by J. McG. Millar was asked to undertake calculations on this point at the Geophysical Laboratory. How serious was the risk from the discarded sabot parts? Conversations with the American Colonels G. M. Ross and B. F. Fellers and the British Colonel J. N. Berkeley-Miller suggested that this risk would not inhibit the use of such projectiles under some conditions.[1] Could more manpower be put on the problem? In August, 1942, the University of New Mexico was awarded a contract for the design and development of subcaliber projectiles under the direction of Dr. E. J. Workman. Ultimately $230,000 was allocated for the work there.

Both the Critchfield and the Workman groups were provided with a 20-mm. Hispano Suiza gun which was a convenient and economical size for testing the sabots. The New Mexico group did most of its early development work, however, with a 6-pounder, Mk VII gun.

By the end of December Workman could report that various designs of sabot projectiles had already been developed and were adaptable to nearly all existing guns as well as being suitable for mass production. The report stressed the advantages of the sabot projectile in its greater chance of scoring a hit on a moving target and its superior armor-penetration qualities. Critchfield however in a theoretical analysis[2] stated that a significant increase in armor penetration was to be expected only if the projectile were made of tungsten carbide.

Sabots were designed and tested in a wide variety of types and materials,

[1]Greig, in a letter to Briggs, dated August 3, 1943, reported talking to Colonel Berkeley-Miller, just after his return from North Africa; the latter declared that a sabot projectile for the tank guns "would have been of the utmost value and the flying pieces of the sabot would not have interfered with its use at all." Colonel Fellers, an Army Officer recently returned from North Africa, later confirmed this conclusion.

[2]NDRC Report A-88: "The Stability of Subcaliber Projectiles."

including plastics, duralumin, and steel. Early test firings for the Geophysical Laboratory were made at the Ballistic Research Laboratory, Aberdeen Proving Ground; beginning December 1, 1942, test firings were made on land leased by the Geophysical Laboratory at Deep Hole Point, Virginia. By May 31, 1943, when Critchfield left the Geophysical Laboratory, experimental models of sabots had been developed for firing 11-mm. and 15-mm. steel projectiles from the 20-mm. gun, at muzzle velocities of about 3500 and 4000 feet per second respectively, at standard pressure, with the same accuracy as the standard round. Plans were underway for a demonstration to the Army.

Some 57-mm. sabot projectiles for a 75-mm. gun were furnished by the University of New Mexico for tests. When fired at Aberdeen Proving Ground in June and July 1943, they proved to have essentially the same accuracy as standard projectiles fired for comparison. These were designed primarily for high-velocity antitank service, and had an armor-piercing shell as a core.

As a safeguard, in case the supply of plastic material proved inadequate, Greig suggested that an all-metal sabot be developed. On invitation C. L. Eksergian of the Budd Wheel Company prepared two designs, and these projectiles were fired at Aberdeen Proving Ground with "moderately satisfactory" results.

The Reviewing Committee on Subcaliber Projectiles was appointed July 20, 1943. Ten days later Burlew made a report to this committee, reviewing progress on the subcaliber projectile for 20-mm. aircraft cannon, developed by Critchfield.

In this memorandum is a description of the steps being taken to complete the design and obtain a manufacturer for 100 of these 20-mm. sabot projectiles for ballistic firing at Aberdeen Proving Ground, to be followed by 5000 more for test of automatic functioning of the gun and another 5000 for further test by the Army Air Corps. Various alterations in design were made to meet material shortages and to facilitate mass production by molding the plastic sabots. (In this design work the Division was assisted by Cummings of the NDRC Transition Office, which was sponsoring this program to get this type of projectile quickly into Service use.)

At Deep Hole Point many of the modified sabot projectiles were fired by the Geophysical Laboratory to determine the effect of variations in the methods of splitting the sabots, and changes in the dimensions of the molded sleeves. Firings also were made of samples made by National Pneumatic Company under a subcontract with the Geophysical Laboratory. In the Laboratory, studies were made of the dimensional changes in the sabots exposed to different atmospheric conditions.

However, the division began to doubt if this sabot could be made to serve useful purposes. This uncertainty, coupled with continuing difficulty in ob-

taining plastic parts molded with sufficiently close tolerances, led to a decision at the division meeting on June 19, 1944, to abandon the 20-mm. sabot project.

However, 57–75-mm. sabots designed and fired at the University of New Mexico had shown increased velocity and flatter trajectory than conventional projectiles. On August 7, 1943, the Reviewing Committee on subcaliber Sabot Projectiles met at the Pentagon with representatives of the Artillery, the Infantry, and NDRC. Colonel S. B. Ritchie stated that the sabot projectile had been of some value to the Ordnance Department, in providing for the first time authoritative information on the increased velocities and accuracy obtainable.

However, the consensus of the representatives of the Armed Forces present at this meeting was that the use of sabot projectiles in 75-mm. guns on tanks was not advisable, because of the danger of sabot fragments hitting our own troops operating in connection with these tanks; accordingly attention should be directed to development of a sabot projectile for use in the 105-mm. howitzer, M3, a short, light gun of low velocity (1,020 feet per second) used by airborne troops.

The objective was to provide more accurate fire from this short howitzer. Accordingly, work on the 75-mm. sabot was discontinued and a contract for construction of 1000 of this size was canceled. Work on the 105-mm. sabot was authorized to be started at the University of New Mexico, to develop a relatively high-velocity armor-piercing projectile. Two designs were successfully developed; but the plastic sabots in moist climate, as at the firing range at Fort Benning, Georgia, proved inaccurate, and the work was therefore discontinued. In the dry air of New Mexico no such difficulty had occurred.

Toward the end of 1943 it was concluded by the Ordnance Department that the sabot projectiles developed for the 75-mm. tank gun could be modified slightly to serve in the 75-mm. pack howitzer. Thus the sabot would readily provide greater velocities and striking power for this low-velocity gun, which had been designed for disassembling and packing into mountainous areas, such as encountered in the Italian campaign.

During 1944 successful firings were made of sample sabot projectiles for the 75-mm. pack howitzer, and high priority was given by the Army for further development. In October the Army expressed its added interest in these sabots, which were expected to see service in the Pacific. This hope, however, remained unrealized when hostilities ceased.

Combat experience had shown the necessity for providing tank guns of larger caliber and higher velocities. The first of these was the 76-mm. gun, which was followed by the 90-mm. gun; the latter had been developed originally as a high-velocity antiaircraft gun. The need for hypervelocity, Division 1 was convinced, could readily be filled by using sabot projectiles

with existing guns. The Ordnance Department requested the participation of Division 1 in this new development, and at a conference between S. Feltman of the Ordnance Department, Dr. C. E. Hablutzel of the University of New Mexico, and Greig, a program for a 76-mm. project was drawn up.

A major difficulty of this new venture was that the 76-mm. gun was to be equipped with a muzzle brake,[3] which meant that either the sabot or the brake had to be changed. At the University of New Mexico extensive experiments were conducted with the 75-mm. gun already on hand, in an effort to perfect a radically different type of sabot projectile that would not break up until it had safely passed through the muzzle brake.

At a conference which reviewed the success recently attained at the University of New Mexico in the development of a sabot for a gun with muzzle brakes, the Ordnance Department representatives reaffirmed their interest in having Division 1 continue its development of the sabot; in addition they suggested that it develop a sabot for the 90-mm. gun, also equipped with a muzzle brake, and produce test lots of the sabot projectiles already developed for the 75-mm. and 105-mm. howitzers. These were to follow the design of sabots with plastic parts, which were to be replaced by light metal parts because of the recent tests which had shown that plastics then available absorbed enough moisture during storage of the projectiles to cause them to swell so much that they would no longer fit the gun. Furthermore, increasing production had made available large quantities of light metals.

Work was continued at the University of New Mexico on the sabots for the 76-mm. gun equipped with muzzle brake. But considerable expansion of other facilities was required to carry on the above program, and on May 9, 1944, a contract[4] was entered into with Remington Arms Company, Bridgeport, Connecticut, for development and construction of sabot projectiles of types to be agreed upon. The 76-mm. and 90-mm. metal sabots were to be produced to fire through muzzle brakes.

Hablutzel, from the University of New Mexico, which made its design for the 76-mm. sabot available to the Remington Arms Company, spent a month at the latter's plant transplanting the University's experience gained during the preceding two years.

The testing facilities of the Ordnance Research Center, Arms and Ammunitions Section of the Materials Branch, were utilized by Division 1 for the initial tests of the sabot projectiles developed by Remington Arms Company. The wartime demands on these facilities at Aberdeen Proving Ground were not favorable to the firings of a research nature. During the

[3]The muzzle brake is a device for decreasing the force of the gun's recoil after firing, the expanding gases at the muzzle being deflected sideways.

[4]OEMsr-1368.

earlier firings the penetration qualities of the sabot were investigated over short ranges prior to conducting stability, time-of-flight, and accuracy tests.

A large number of test projectiles were considered necessary before it could be fully known whether a projectile had inherent accuracy. Arrangements had to be made to close the adjacent firing ranges so that the fired projectiles could be recovered. It became more apparent after this recovery that the sabot project would assume a purely developmental aspect, rather than one of engineering to allow early mass production.

A search for a suitable range disclosed that the idle artillery ranges at Pine Camp (near Watertown, New York) would be most satisfactory from all standpoints. Arrangements were then made through the Office of Chief of Ordnance for the assignment of "F" range at Pine Camp for the testing of sabot projectiles.

This range offered a flat, open space from the gun position to the 2100-yard target. After permission had been granted by the Second Service Command for use of this range, facilities for testing the 90-mm. projectile were installed, with the exception of armor-plate penetration tests. The physical facilities for the range were set up with Army issue equipment wherever possible, and these were augmented with purchased equipment as required.

The development of a sabot for the 105-mm. howitzer was considered of paramount importance; five other types and sizes were arranged in the desired order of priority, but this arrangement was later changed several times. The active project subsequently was reduced to the 90-mm. and 76–57-mm. sabots.

Some 30 designs and 537 samples were produced in the evolution of the deep-cup middle-bearing sabot, which offered the minimum manufacturing and functional difficulties for a sabot required to fire through a muzzle brake. A 6-pound unground tungsten carbide core proved most promising, but further tests were expected to show that an 8-pound model would give improved stability and resistance.

Test firing of an improved model of the 90-mm. sabot showed that it had the desired penetration qualities but was inaccurate. This was attributed to several causes, principally erratic separation of the sabot parts in flight, even though micro-flash photographs showed consistent release between 10 and 12.5 feet beyond the muzzle.

The belief that accuracy was chiefly a function of external release prevailed until, through OSRD liaison with the British Army, a meeting was held with the scientific personnel at Valcartier Proving Establishment, Province of Quebec. This group held the theory that accuracy was developed within the gun bore rather than externally. This theory was supported by extensive data compiled by the Canadian Army during acceptance proof firing of some 300 lots of sabot projectiles.

Following this lead the problem was approached from an entirely new angle. Each element of the sabot was studied to determine whether it was strong enough to withstand the large stresses imposed while in the gun tube. Firing tests of modified designs confirmed the conclusion that a weak rotating band, which permitted the projectile to "wobble" in the bore, had been partly responsible for the inaccuracy.

All trouble from inaccuracy was finally eliminated by another change in design introduced about this time which eliminated the possibility of interaction between parts of the sabot and the subcaliber projectile at the time of break-up of the sabot. It also helped strengthen the sabot while in the bore.

Thus the final answer to the problem of inaccuracy was not obtained until various elements had been brought into proper relation to each other, another proof that the attainment of stable flight by a projectile requires the most delicate balance among the interrelated factors.

In a letter to Paul A. Scherer, Chief of the Engineering and Transition Office, NDRC (dated March 21, 1945), Adams submitted data on the penetrating power of the 76-mm. sabot projectile, and pointed out:

> From these figures, it may be seen that the 76-mm. sabot-projectile is able to defeat the side armor of German Tiger tanks at ranges greater than 2000 yards, whereas the frontal armor, being somewhat thicker than 5 inches, cannot be defeated at ranges much greater than 1500 yards.

Adams also noted the "considerable advantage" of such penetration as compared to that of the Ordnance Department's new "half-weight" projectile with a tungsten carbide core, comparable in diameter and weight with the core of the sabot projectiles. He also submitted information from Europe which supported the division's position as to the need for high-velocity ammunition:

> The above information is being submitted because I believe it will be of interest to you and Mr. Wagner[5] in connection with the point of view brought back by Mr. Wagner from the European front, to the effect that the Armored Forces are now experiencing a real need for a projectile that can be shot from *present* U.S. guns and that can cope with the German Tiger tanks.

By March 31, 1945, the Ordnance Department was showing much more interest in sabot projectiles. In a letter of the above date to Adams, Colonel Ritchie expressed the Ordnance Department's revised attitude in these terms:

> In view of the increased penetration of armor plate that the sabot-projectile offers, especially at greater ranges, it appears highly desirable that its development be pursued vigorously in an effort to improve its accuracy.
>
> It is, of course, difficult if not impossible to justify the use of this type of ammunition unless the design is such that it lends itself readily to mass production

[5]E. M. Wagner, Chief, Engineering Office, NDRC.

and also one which can be fired at short and relatively long ranges with small dispersion, making it an accurate shot against point targets.

In view of the desire to obtain increased armor penetration, it is requested that, in so far as possible, efforts be concentrated on the development of a satisfactory sabot-projectile to be fired from the 90-mm. tank and antitank guns equipped with muzzle brakes. Particular attention should be given to the improvement of the accuracy.

This formal expression of the Ordnance Department's interest in the sabot projectile confirmed the program of Division 1, which it had undertaken on its own initiative and judgment.

The division's final conclusion was that the sabot projectile offers a practicable method for obtaining high velocities from conventional guns, with satisfactory performance as to accuracy. At least for emergencies these qualities should be potentially useful, as they would enable gun crews to switch rapidly from standard to high-velocity ammunition; during a sudden tank attack, for example, this quick increase in fire power and penetrative ability could be of real military value.

## Tapered-Bore Gun and Deformable Projectile

One of the earliest Division 1 contracts (its fourth) was made as of April 15, 1942, with Jones and Lamson Machine Company, Springfield, Vermont, for the design, development and manufacture of a 57/40-mm. hypervelocity gun using the tapered-bore principle. Shortly afterward the Bryant Chucking Grinder Company was called in to work out the principles and design of a projectile for this gun.

Early types of hypervelocity guns included the Gerlich tapered-bore gun developed and used extensively by the Germans, and the Janacek tapered adapter applied to a straightbore gun, developed during the early stage of the war by the British under the code name "Littlejohn." Each of these uses a skirted, deformable projectile adapted to change with the diminishing diameter of bore between breech and muzzle. The tapering bore squeezes the skirt to a considerably smaller diameter, and the projectile, being abnormally light, attains much higher velocities than the conventional projectile.

Tests had been made as early as December 1932 at Aberdeen Proving Ground on a tapered-bore rifle invented and offered for test by H. Gerlich, a German; a velocity of 4400 feet per second was obtained. Later the Ordnance Department had built a caliber .30/.24 rifle along the same lines, achieving 4100 feet per second. Accuracy was not up to our Army standard, and the idea was abandoned. But the Germans applied the Gerlich invention to larger guns, and by 1942 their 28/20-mm. tapered-bore guns, which had velocities over 4000 feet per second, were reported to be doing great damage to the Allies in North Africa, particularly against tanks.

In the Gerlich gun the tapered transition from the initial large caliber to the emerging small caliber occupies most of the length of the gun and the bore is therefore necessarily rifled throughout its length. The production of such guns is laborious and expensive in man and machine-production hours. Our ordnance men were unable to comprehend why it was adopted by the Germans in preference to the Janacek design, which presents much simpler production techniques requiring neither special machinery nor laborious operations.

The Jones and Lamson studies assumed that if projectiles could be developed to stand the shock of rapid deformation the tapered section could be made quite short; rifling then would be unnecessary, because of the negligible loss of spin, and the Janacek adapter would be preferable to the single-piece rifled and tapered gun tube, because of the greater simplicity of manufacture. The adapter can readily be applied to any standard gun tube of suitable rifling pitch, and possesses the additional advantage that it is readily replaceable when worn out by the violent friction developed during the "squeezing" of the projectile.

Since the final determination of the optimum forms of gun design was dependent on the determination of projectile limitations, it was necessary to concentrate during the early days on the perfection of projectile designs. At the same time it was necessary to have guns in which to test experimental projectiles. So Jones and Lamson pushed the design and production of tapered guns and the tools to make them with, while Bryant studied the theoretical aspects of projectiles and worked out preliminary designs.

The success of the Bryant Chucking Grinder Company's efforts to produce a projectile capable of withstanding the violent changes during the deformation, which takes place in approximately one ten-thousandth of a second, was in no small measure responsible for successful over-all attainment of a practical weapon.

The immediate background for projectile work by the Bryant Company was furnished by the Gerlich ammunition, by some British development directed toward improving the Gerlich design, and also by work which was being done for Section A–A by L. R. Sweetman at the National Bureau of Standards. The test firing of these projectiles was carried out at Aberdeen Proving Ground.

During the initial stages of this project study was made of early patents on tapered-bore guns, including British and German patents and descriptions running back to 1888. Secret British Intelligence reports, and captured German guns tested at Aberdeen Proving Ground, provided useful information.

Bryant learned that some types of British ammunition had a tendency to break up on firing, a trouble which had not been overcome. Also, the British design proved quite complex, involving numerous parts. Mr. Sweet-

man's work at the Bureau of Standards proved useful, his design of projectile being relatively simple and easily made.

Starting on this foundation, Bryant engaged in the difficult project of developing a deformable projectile suitable for use in a hypervelocity tapered-bore gun, hoping to overcome design difficulties which at that time limited the practicability of the weapon's use. The company eventually produced a satisfactory design for such projectiles, and established principles which were considered suitable for such projectiles of almost any caliber, as well as for the specifically assigned 57-mm. tapered-bore gun with 40-mm. emergent caliber.

The joint arrangement between this company and the Jones and Lamson Machine Company continued until December 31, 1943, with essentially the same research personnel carrying on the work for both projects. As of this date business and administrative factors made it desirable for the Jones and Lamson Machine Company to take over the entire operation, using most of the personnel previously employed under both contracts. At this point there remained the problem of designing deformable projectiles in accordance with the principles developed by the Bryant Company, involving the incorporation of an armor-piercing core and the perfection of the over-all design for the attainment of optimum armor-piercing performance.

The deformable projectile was designed with the same general objective as was the sabot projectile, that is, to attain hypervelocity by reducing the diameter and weight of the projectile, and in addition achieving maximum penetration of armor by a design whereby the mass is concentrated toward the center of the projectile. Two deformable skirts were attached to, or machined integrally with a steel sheath which contained a hard, high-density core, such as tungsten carbide. In addition, a steel or aluminum wind-shield was attached to improve the flight characteristics.

As compared to the sabot projectile, the deformable projectile offers two definite advantages: (a) it has no extraneous parts to scatter in front of the gun; (b) all the initial momentum of the projectile is retained, resulting in better ballistic characteristics in flight.

For testing the guns and projectiles a 100-yard gun range was constructed three miles from the Jones and Lamson plant, in a deep gully. This range was equipped with a gun room, a chronograph and observation room, loading room, workroom, powder vaults, recovery butt, and a trailer for housing the delicate strain-gauge instruments.

Measurements were made of projectile velocities, strains in gun tubes and adapters, maximum powder pressure, tube and adapter wear. Stands were provided every five feet along 200 feet of the range, to hold yaw cards for determining flight stability. A micro-flash unit was used to obtain pictures of projectiles in flight, and shadow pictures for determining projectile wave fronts.

Ten experimental tapered-bore guns were produced in pairs by Jones and Lamson with tapered lengths of two inches, four inches, eight inches, sixteen inches and thirty-two inches respectively. One of each pair was rifled through the tapered section as well as the large bore while the other was left smooth in the tapered part of the bore. At a very early stage in the test program it was demonstrated that projectiles could be designed successfully to pass the four-inch and longer tapers without breaking up, and that with the eight-inch and shorter tapers the loss of spin velocity in traversing a smooth, tapered muzzle section was negligible so far as its effect on the stability of the projectile flight was concerned.

Jones and Lamson's original theoretical "hunch" was proved sound: it was clearly established that the best way to build a tapered-bore gun was to make it in two parts. The main part could be a standard gun of suitable rifling pitch. The special part would be small, easy to handle and easy to machine. Thus, the need for expensive, slow, special production machinery was eliminated.

There remained only the problem of joining the two parts in such manner that they could be readily assembled in the field without special tools. This step was not difficult. Attachment of the adapter to the gun was accomplished with a loose coarse thread and suitable centering and steadying bearing surfaces. When the designs had been completed, it was found that performance was perfect when the parts were merely screwed together by hand and, rather surprisingly, it was found that they could be unscrewed by hand even after the firing of many rounds.

To surmount the last engineering problem on the road to practical application, simple methods and tooling were conceived for ready modification of guns in service, so that they might receive adapters in the field.

To decrease wear and extend the life of adapters, several experiments were tried but none was carried far enough to yield useful results. The final report from the Jones and Lamson Company recommended that further attention be paid to reducing wear.

The Company's work resulted in velocities of 4200 feet per second with carbide-cored projectiles, and 5200 feet per second with lightweight solid steel projectiles, within the rated maximum powder pressure of the gun. Penetration of armor at such velocities proved very high. Accuracy was superior to that of the sabot projectile, chiefly because the tapered-bore projectile is accurately guided as it leaves the gun.

Division 1 was able to report that its design of gun and projectile produced far superior results, ballistically, to those of the German Gerlich guns. The latter's ballistic properties were reportedly so poor as to nullify all its advantages beyond a few hundred yards' range; also, the velocities attained exceeded those of the captured Gerlich guns, and the accuracy was far above that of the German guns.

## The Pre-engraved Projectile

Division 1's initial interest in pre-engraved projectiles started early in 1942. From the published history of the big gun that the Germans had used in firing on Paris during World War I it was realized that pre-engraved projectiles represented one means of withstanding the excessive engraving stresses produced in conventional rotating bands, under hypervelocity conditions. The conventional copper rotating band that is satisfactory at ordinary velocities is not strong enough at velocities above about 3500 feet per second. In 1942, however, the division had no hypervelocity guns and therefore was not in a position to experiment with these projectiles. The first such experiments were carried out a little over a year later at the Franklin Institute, where the .50-caliber erosion-testing gun then was being used for a wide variety of tests.

Pre-engraved projectiles were first tried with this gun in an effort to separate some of the factors involved in erosion. It was felt that by the use of pre-engraving the friction factor in erosion would be largely eliminated; the corresponding reduction in erosion of the gun steel barrel was even greater than had been anticipated. There followed a long series of careful tests involving variations in the designs of the projectiles and the conditions of firing. The outstanding result was the discovery that when the bore surface of the barrel is chromium-plated, the use of a pre-engraved projectile is several times as effective as if the barrel were not chromium plated. Parallel experiments performed at the Geophysical Laboratory at about the same time with chromium-plated .50-caliber machine-gun barrels demonstrated that one of the major causes of the removal of chromium plate from such a barrel was the flattening of the steel beneath the plate. This evidence confirmed the desirability of employing some means of reducing the engraving stresses when a chromium-plated bore surface was used.

The proposed plan to decrease erosion by the use of pre-engraved projectiles in a chromium-plated bore was not considered a practical solution of the problem by the representatives of the Army and Navy. They pointed out that the trend in armament was for rapid-fire guns, even for calibers as large as 120-mm. For such guns it would be necessary to devise some means of mechanically orienting the pre-engraved projectile during loading, so that the teeth on the projectile would engage with the grooves in the barrel. In the tests at the Franklin Institute each projectile was carefully inserted by hand; but such a procedure was out of the question in battle for any except large-caliber guns.

During rapid firing, it had been pointed out, a single case of misalignment would be fatal. This potential danger led to a study of loading mechanisms and the possibilities of indexing the pre-engraved projectiles. Visits were made to Army establishments to study rapid-firing antiaircraft

guns in actual operation, and certain tests were made there to determine just what had to be done in order to index the projectiles.

The importance of this phase of the problem was emphasized in the report of the Resistant Materials Committee on June 1, 1944. That committee recommended that assistance should be obtained from qualified designers familiar with the development of special machinery performing intricate operations, such as sewing machines, knitting machines, special harvesting machinery or the machinery used for packaging and wrapping materials.

A conference on pre-engraved projectiles was held June 17, 1944, with representatives of the Ordnance Department. Future plans were discussed, and a proposal by representatives of the Franklin Institute was finally adopted. It provided for making a high-velocity 37-mm. gun from a 40-mm. forging, using the standard 40-mm. chamber and cartridge case necked down to the 37-mm. size. The Ordnance Department expressed considerable interest in trying pre-engraved projectiles in a 90-mm. gun. A Division 1 representative argued strongly that even the preliminary work should be done for this size instead of for the 37-mm., but it was finally decided that a smaller size might be dealt with more quickly.

The Franklin Institute prepared the designs for this new gun, which was designated by the Ordnance Department as the "T-47." TenBrook, the division's newly acquired engineering adviser, was given the assignment of helping the Franklin Institute find adequate facilities for doing this work. Some 40-mm. forgings obtained from the Navy Department were bored, chambered and rough-machined at the Chrysler Plant, then shipped to the Naval Ordnance plant at Charleston, West Virginia, where they were rifled.

From there the tubes were sent to the Naval Gun Factory in Washington, D.C. for chromium-plating. This operation was carried out under Blum's supervision. After having been returned to the Charleston plant for honing, the tubes finally went to the Franklin Institute for fitting to the mounts that had been prepared there by modification of standard 57-mm. mounts.

Delays occurred in each of the many steps of this involved procedure. Consequently the first 37-mm. tube was not ready for firing tests until nearly a year later. One of them was given an erosion-life test at Aberdeen Proving Ground with disappointing results. The chromium plate was removed rapidly from the origin of rifling, apparently because of the action of the hot powder gases sweeping past the lands, which had been pointed at the ends in order to facilitate indexing the projectiles. This result emphasized the importance of the "scale factor" in ordnance design, for no similar difficulty had been encountered in the caliber .50 test of the barrels with an analogous design.

Another one of the tubes was sent to the Division 1 firing range at Carderock, Maryland, where it was used in an interesting series of ballistic firings conducted by the National Bureau of Standards and the Geophysical

Laboratory. These ballistic measurements were rapidly carried out during the summer of 1945, by means of special measuring devices that had been developed during the previous two years of firing tests with the 3-inch gun.

In the meantime the Franklin Institute had given some attention to the matter of designing a loading mechanism for pre-engraved projectiles, by modification of the standard 40-mm. Bofors loading mechanism. A full-scale model of such a mechanism had been completed by the time experimental work under this contract was stopped at the end of October 1945.

In the spring of 1945, before the 37-mm. gun had yet been fired with pre-engraved projectiles, Division 1 decided to make use of this method of erosion control in the design of the special 90-mm. hypervelocity gun described in the next chapter.

Although the Division has not completed the solution of the problem of correct orientation of the projectile during loading, it considers this problem capable of solution. In view of the great reduction of bore friction to be obtained by the use of the pre-engraved projectile, particularly in the chromium-plated bore, it is evident that this device offers a useful approach to future design of hypervelocity guns with reasonably long life.

CHAPTER XXXVI

# THE GUN OF THE FUTURE

DIVISION 1, in order to reach its assigned objectives of increasing the velocities of projectiles, required much better knowledge as to the causes of gun erosion. This in turn required detailed information as to what goes on in the gun barrel during firing. Actual guns, it was realized, fall far short of the ideal gun, in that much of the available energy of the propellent powder is not transformed into kinetic energy of the projectile. A major cause of this loss, which becomes especially important for hypervelocity guns, is the kinetic energy of the powder gases that follow the projectile.

Two other principal causes of failure to reach the ideal were found and evaluated during the division's research: these are, first, thermal losses to the bore, that is, heating of guns as a result of firing; and second, bore friction, including the various forces that oppose the acceleration of the projectile. Knowledge of both these factors was advanced greatly as a result of the division's long series of experimental and theoretical studies.[1]

## ADVANCING THE ART OF INTERIOR BALLISTICS

The final reports of Division 1 investigators give in some detail the highly useful data that were acquired on these two causes of diminished performance in new guns. Thus as to heating of guns during firing it was reported in Chapter 5 of the Summary Technical Report of Division 1:[2]

> In attacking the problem of hypervelocity, the direct approach would seem to be that of increasing the energy of the powder charge. This could be done quantitatively by increasing the amount of powder, or qualitatively by using a powder with higher potential. The limitations of the former method are well known. When the latter method is used we encounter higher flame temperatures, and the resulting temperature of the gun bore becomes seriously high. If we combine with this the high rates of fire maintained in automatic weapons, the question of heating of guns during firing is critical. The thermal effects are an important factor in erosion, and in addition lower the tensile strength of the gun metal. Division 1 was therefore interested in the study of the heat input to the gun,

[1]Attention has been directed previously in this history to the difficult and complex nature of measurements concerned with gun-barrel reaction. It was necessary to record pressures up to 50,000 pounds per square inch, and temperatures of 3500° F. or higher, during firing periods that were measured in thousandths of a second.

[2]By Dr. H. L. Black and Dr. George Comenetz.

the distribution of heat in the barrel, the temperature of the bore surface, and in methods of gun cooling. . . .

As a result of these studies, our knowledge concerning heat input from a single round, input from a burst or a series of bursts, temperature attained by the bore surface, critical temperatures in firing, and methods of cooling has been greatly extended. The application of this knowledge in the development of improved machine gun barrels in lined and/or plated guns and in medium caliber automatic guns, has been of definite value, and will aid in the design of future hypervelocity guns.

From the theoretical point of view, a better understanding of the heat input from band friction and its combination with that of the powder gases is to be desired. Similarly, the effect of thermal stresses taken in combination with band and powder pressures should be more completely analyzed as an aid to the designer. We may expect improved experimental methods of measurement of the bore surface temperature.

To combat the effects of heat input we may hope for the development of alloys with physical qualities which will make them satisfactory as gun metals at temperatures above the melting point of steel. Finally, methods of cooling under service conditions will certainly be rapidly improved.

Results of investigations of bore friction were given in another chapter[3] of the division's final Summary Technical Report. Bore friction, it was pointed out, not only is an important cause of diminished performance of new guns, but is "a factor of some importance in their erosion, and a prime factor in their erratic behavior." Methods were developed, including gauges for direct measurement, to determine the total accelerating pressure, the base pressure on the projectile, and their difference, which is the resisting pressure due to bore friction.

Measurements of these and other quantities, often involving verification of calculations, were made as part of the Division 1 program at Carderock, Maryland. There a 3-inch Navy gun was equipped with a variety of instruments for registering the important events which occur when a gun is fired.

This laboratory gun was covered from end to end with an array of timing devices, strain and pressure gauges, and various other measuring and recording instruments. As many as 17 different events were recorded during the extremely brief interval of time while the projectile was moving down the gun bore. An ingenious "quartz glass" window in the barrel enabled photometric observations and recordings to be made of the temperature of the powder-gas explosions. It was found that while the projectile was being engraved the frictional force was so high that it could be measured with reasonable accuracy.

The techniques that were developed by the firings with the 3-inch gun were later used for a series of ballistic measurements during the firing of a

[3]Chapter 6, "Bore Friction"; Dr. William S. Benedict.

37-mm. hypervelocity gun that used pre-engraved projectiles. This was the first time that information about the interior ballistics of a gun firing such projectiles was obtained.

The above-mentioned report on bore friction concluded with the statement that further work along this line, "using a variety of guns firing conventional and experimental projectiles and powder is needed. Only in this way will it be possible to establish the general laws relating bore friction and powder energy distribution to interior ballistic theory and to gun design and performance."

## Interior Ballistic Calculations

A project of great value to Division 1 was inaugurated in January 1942, when Dr. J. C. Hirschfelder, Assistant Professor of Chemistry at the University of Wisconsin, was appointed a Consultant to Section A–A, to initiate a theoretical study of the composition and temperature of the powder gases, and of the temperature of the bore surface. For this purpose he introduced some new features into conventional methods of making interior ballistic calculations, and these led to the development of an improved system of calculation.

The underlying purpose was to obtain a better understanding of gun erosion, and to improve the performance of hypervelocity guns. In 1941, when the division's work along these lines was undertaken, little was known about the temperature of the bore surface of guns, or of the temperature and composition of the powder gases reacting with it. Information as to the latter was obtained experimentally by Dr. F. C. Kracek and Dr. W. S. Benedict by means of a closed pressure vessel, and later in the 3-inch gun at Carderock, with close agreement with calculated values.

In June 1942 Hirschfelder joined the staff of the Geophysical Laboratory. There a large group of computers under his direction were kept busy many months preparing mathematical tables and charts to simplify ballistic calculations; this group also solved ballistic problems for other contractors of the division.[4] Hirschfelder left in the fall of 1943 to join the atomic bomb research group, and Dr. Richard B. Kershner, formerly Assistant Professor of Mathematics at Johns Hopkins University, took charge of the interior ballistics group.

Interior ballistic calculations deal with the variables that the mathematician puts in the equations that he uses to describe how fast the projectile moves down the bore of the gun, and how long it takes to do so. These variables are so numerous that it is difficult for the ballistician to predict exactly the outcome of firing a given gun with designated ammunition, which is what he would like to be able to do.

[4] This work turned out to be especially valuable to those developing propellants for rockets. See Part One.

Accurate knowledge of these variables is required for such practical activities as predetermination of muzzle velocity, considered to be the most important property of a gun; development of methods for determining whether a prospective powder may be safely used in an existing gun; design of a gun of minimum weight that will not burst.

Such knowledge also was needed in the study and design of gas-operated rapid-fire guns, and, of course, in understanding the thermal and chemical processes causing erosion. The data also are useful in making calculations for recoilless guns, as well as studies of blast and flash of guns.

In this endeavor the Geophysical Laboratory developed a formalized method of setting up and solving the equations covering the determined properties of the powder and gun, and their relations in accordance with the general laws of physics. The advantage of this "system of interior ballistics" over previous systems "consists primarily in the large number of factors given explicit consideration, and related *a priori* to the powder composition."[5]

Various sets of tables were prepared which greatly reduce the difficulty of solving many types of problems. These tables were useful in the design of a 90-mm. gun on which Division 1 began construction; this gun is described in a succeeding section of this chapter. Application of the principles to the design of conventional guns and projectiles, in an effort to increase velocities to nearly 5000 feet per second, showed that such velocities were difficult to attain unless very long weapons were used. An advantage of these ballistic tables, as compared with the ones previously used by the Services, was that they could be used for calculations concerning the performance of unconventional projectiles, such as sabot projectiles and those fired from tapered-bore guns.

The division's work in interior ballistics resulted in increased knowledge which, together with the development of new techniques, is expected to lead toward considerable improvement in future ordnance design.

## The Flying 20-mm. Cannon

In the summer of 1942 the Navy Bureau of Ordnance requested NDRC's assistance in improving the Hispano Suiza 20-mm. aircraft gun. Refined and adapted for aircraft use by the Army Ordnance Department, the gun's assist-feed mechanism for wing installation up to that time had not proved entirely reliable, besides occupying excessive space. The need for a 20-mm. gun in aircraft had been well established in combat service.

Division 1, although realizing that this assignment was not exactly in its line, concluded that development of a 20-mm. gun of rugged construction might open a new approach to the perfecting of a hypervelocity machine gun. Johnson Automatics, Inc., called in for consultation, suggested that the best solution would be a completely new self-actuating gun.

[5]Dr. William S. Benedict, "Interior Ballistic Calculations," Chapter 3, STR.

Under date of October 8, 1942, the company was given a contract to proceed with the design, construction and testing of an improved automatic aircraft cannon, as an "essential for success in aerial warfare," under general specifications provided by the Navy Bureau of Ordnance.

When the first model was tested only short bursts were fired. They revealed the need for redesigning several parts, and also proved that the standard Oerlikon ammunition was unsuitable for use in a gun that operated on a different principle from the one for which it was designed.

Additional personnel and facilities were obtained for designing and experimental work, to expedite solution of various difficulties that still remained. Service interest in the new design meantime became somewhat less intense, partly because of the rather slow progress, and also because the Army and the Navy now each had two parallel projects of their own, including further improvements in the Hispano Suiza gun.

It was not until nearly a year after the project had been started that Division 1 received from the Navy Bureau of Ordnance detailed Tentative Specifications covering the desired characteristics for the 20-mm. gun, including details on ammunition, rate of fire, feed mechanism; size, weight, and muzzle velocity; case and link ejection, control, mounting, lubrication, conditions of operation and other essential requirements for a gun of this type.

An improved version of the first model was fired in short bursts at the Geophysical Laboratory 20-mm. test pit. Design of a second model was then begun to embody all the lessons previously learned; this model was not constructed, because during the long period involved in the preparation of drawings for it, firing tests were being conducted with Model I. At one time a burst of 41 rounds was obtained with Model I at rates of fire varying from 513 to 531 rounds per minute. High-speed motion pictures proved very useful in these tests.

The knowledge gained from the additional firing experience with Model I was embodied in a third model which was radically modified to include all features of the revised specifications, improved type of ammunition, and various design changes that had been made. Model III was fired for the first time at the Diamond Hill Firing Range, Providence, R. I., testing facilities which had been obtained.

Although the division and the contractor exerted considerable effort in this project, lack of suitably trained personnel retarded progress during much of the period involved, and it proved impossible to complete a practical, usable 20-mm. automatic gun before the war ended. It may be noted that in the experience of ordnance engineers the design of a new type of automatic gun is expected to require from five to ten years of designing, modeling, testing, discarding, redesigning and rebuilding.

At the end of the contract the division and the contractor were able to report overcoming the two toughest obstacles, action and feed system, and

were satisfied that the basic design is sound. It is believed that a fourth model embodying all previous experience should be satisfactory in every way.

## The Super-Gun of the Future

At various times Dr. G. S. Fulcher and others in the division had advanced the idea that "the best way to make progress on a hypervelocity gun was to construct one and study it." It was not until 1944, however, that any plans were made to do so. In January of that year, at Burlew's suggestion the amount of $200,000 for building an experimental hypervelocity gun was included in the division's budget request for the coming fiscal year.

Then, toward the end of March, S. Feltman, the division's principal liaison in the Ordnance Department, requested Division 1 to indicate its ideas on preventing excessive erosion in a 3-inch gun when fired at an assumed muzzle velocity of 4000 feet per second, Burlew took the opportunity again to advance the idea of building the super-gun. Although understanding that Feltman was thinking of a gun for the next war, he pointed out to Adams that the Ordnance Department, having turned to Division 1 for aid in erosion control, might support a definite hypervelocity project; in fact it seemed to Division 1 a golden opportunity to achieve its goal of a hypervelocity gun for Service use. Furthermore, Burlew pointed out, "I believe that if this challenge is picked up by us, so that we exert sufficient co-operative effort, we can make this dream come true in time to help to win the present war."

Plans prepared by Burlew were considered by the Division Members, and a special committee under the chairmanship of Rose was set up during the summer to decide on the scope of the project. The demands of the swiftly moving Stellite liner program, coupled with the fact that the requested funds were not included in the budget, retarded progress for several months. Interest was revived by the end of the year and the committee presented a report in January 1945 in which the objective of the proposed project was declared to be "to provide a working model of a hypervelocity gun as an example of what could be done in a practical way toward increasing the muzzle velocity of guns." Although there was some doubt that the gun could be built in time to see service in the war, it was agreed that a gun incorporating everything that had been learned by the division would be very useful as a basis for future research in hypervelocity weapons.

On March 20, 1945, a new Project-Control Committee[6] termed the A–Z was formed and vigorous action ensued. The plans had by this time crys-

[6]The Chairman was TenBrook; Vice-Chairman Burlew; Technical Aide (pro tem) Shallenberger; division member, Rose. Division Deputy Chief Allen was member ex officio of the Committee.

tallized on a caliber of about 90-mm. and a muzzle velocity of at least 4000 feet per second. As already mentioned in the previous chapter, it was decided that the combination of pre-engraved projectiles and a chromium-plated bore was the most promising means of controlling erosion then available. Adams and Paul A. Scherer, Chief of the NDRC Engineering and Transition Office, who was called in to help find engineering aid, both stressed the desirability of making a determined effort to make a workable gun at the first attempt, while at the same time counting on a relatively long-range program of continuing improvement. This latter was to include an exploration of means of utilizing extremely high chamber pressures and higher densities of loading of the powder in the cartridge case.

In June NDRC allocated $185,000, and various details were agreed upon, a program being adopted for designing and producing the various components of the hypervelocity gun. A subcontract from Franklin Institute with Drexel Institute was approved, involving the services of Professor J. H. Billings of the latter Institute to supervise and co-ordinate all elements of the design. Arrangements were made on June 4 with F. C. Gladek and Son, of the Machine and Tool Designing Company, Philadelphia, for engineering design studies. A contract[7] was made with the Midvale Steel Company to forge and machine six tubes, and the Forked River Proving Ground was obtained for testing the new gun, involving the building of a road and other construction. This was a co-operative project with Divisions 2 and 8, under the administrative supervision of Division 2. In July a prospective contractor was obtained to manufacture the projectiles.

By the time that the Division began to terminate its undertakings in September 1945 the tube forgings had been cast and the designing of most of the rest of the gun was completed. The one difficult problem not yet solved was that of an automatic loading mechanism to index the pre-engraved projectiles. The Midvale contract was continued into 1946 so that the tubes might be completed. Finally, in April 1946, two of the tubes were delivered to Watervliet Arsenal, where they were to be used in continuation of the hypervelocity-gun project by the Army Ordnance Department.

## Division 1's Contributions to Future Gun Design

The application of gun-erosion knowledge to actual gun design, as in the hypervelocity gun, was analyzed by Rose, Division Member. He pointed out that the Division's advances in ordnance knowledge should be important in two ways: first, in influencing favorably the ballistic performance of future guns; and second, in making possible the production of guns with increased military value.

The ballistic improvement will result in higher muzzle velocities. There is, of course, considerable cost in money and in weight to this increased

[7] OEMsr-1499.

velocity and firing effectiveness. The present muzzle velocity is close to 3000 feet per second for antiaircraft use; 7000 feet per second might be obtainable at very high cost. Velocities of 3500–4500 are obtainable at rather reasonable cost in expense and weight but even that involves increasing the caliber-length of guns; the present standard length of 35 to 60 calibers must be increased to 80 to 100 calibers. This increases the weight of the barrel, which must go up greatly to yield 4500 feet per second without using new materials.

This means, in effect, boosting the weight of the gun tube and mount. Specifically the powder charge of a 120-mm. gun must be used to propel a 90-mm. projectile. Therefore a 90-mm. gun's weight must be increased to that of a 120-mm. gun. This entails increased cost of ammunition, also higher pressures, new forgings, new projectiles, involving less explosives in the projectile, which reduces the range of destructiveness.

Application of the Division's erosion knowledge to gun design leads into engineering and economic problems involving a balance between the size and velocity of projectile and the gun's firing rate, in order to provide the optimum in terms of destructiveness as measured against military cost. The latter includes cost of shipping space, maintenance of stocks, manpower to produce and handle, up to and including its use in the field, as well as flexibility for various purposes, and other elements of military cost.

Improved guns help to reduce the burden on production and shipping facilities. Better guns last longer, save transportation of spare barrels, reduce the manpower required to produce the guns.

Tending to counterbalance the gains of increased gun life are the loss factors in the field during combat. Loss of materiel, in the military sense, is incurred through destruction by enemy attack, loss in transit or from being shot up, loss by rough handling, muddy terrain, and so on. These loss factors operate against the more expensive high-velocity guns much the same as against standard guns.

## "Swords into Plowshares"

Just as the bow and arrow enabled men to acquire food on the hoof, and later led to the harp, the violin, and the piano, so may the remarkable gun materials, techniques, and processes developed by Division 1 investigators aid peacetime industry in ways that are already definable.

Although the complex reactions occurring during the firing of a gun are not paralleled by any *present* industrial process, the fundamental knowledge which has been gained of such reactions will enable designers to transform future high-pressure equipment, such as internal combustion engines and high-speed turbines, and to improve high-compression technology generally.

There is no doubt that remarkable new erosion-resistant alloys such as machinable "Alloy X," and the metallurgical techniques developed to re-

duce gun-barrel erosion, will be highly useful in numerous future applications. Technical advances will be increasingly wide as further exploration is made of the little-known world comprising the high pressures and high temperatures met with in the search for higher-velocity guns.

An example of many specific advances by Division 1 investigators is the technique of pyrolytic plating from the vapor of metal carbonyls. This process was improved to the point where it could be adapted to commercial operations. It was learned that two of the materials most resistant to powder-gas erosion may be deposited by decomposition of their carbonyls on heated surfaces. At the Bell Telephone Laboratories such vapor-phase coatings were evolved by Dr. L. H. Germer, Dr. J. J. Lander and their colleagues to the point of giving a plate of any desired hardness, up to about 2200 Vickers hardness number — harder than sapphire.

One advantage of this process is that it opens the way to using materials with high melting point and high hardness without the fabrication troubles involved in their use in thicker form. As among such materials are to be found the nearest things to an ideal erosion-resistant material, this is a very attractive possibility.

It was found during these experiments that the hardness of the films is adjustable by controlling the carbide content. This may lead to development of highly useful abrasion-resistant carbide films.

The experimental and mathematical progress made by Division 1 investigators in ballistic science no doubt will be basically useful in early development of rocket transportation, a field with untold potentialities.

There were a great many such advances, which will have repercussions in industry and the arts for years to come. Dr. Karl T. Compton, President of Massachusetts Institute of Technology, a leader in numerous OSRD–NDRC scientific enterprises, during the war made the following appraisal of wartime research in peacetime:[8]

> When victory has been won and the whole story of these scientific accomplishments can be told, it will indeed be a thrillingly interesting recital. Out of it will come, not only its important contribution to victory, but a number of exceedingly significant results of permanent peacetime value. It is already evident that many of these wartime developments will have very useful peacetime applications, whose contributions to our standards of living and general prosperity and comfort will help to compensate for the ravages wrought by the war.
>
> Scientists will have a renewed faith in the worthwhileness of their work, and will continue their intellectual and practical endeavors with the increased power that has come from the experience of 'team-work' on war problems. The general public, and especially the governmental and industrial leaders, will have greater appreciation of the value of science and scientists, both pure and applied, and this should result in permanently increased support of scientific research in the universities, industries and governmental agencies.

[8] "Organization of American Scientists for the War," *Science*, July 23, 1943.

# APPENDIX 1

# CONTRACTORS AND THEIR PERSONNEL

## Mobilizing the Contractors

Some of the country's finest research and commercial facilities were called on for aid, in order to speed up the division's program, and also to obtain the services and ideas of specialists in various lines. Although most of them were then engaged in war work, and later became heavily overloaded with such activities, they devoted their best efforts toward solution of the Division 1 problems assigned to them.

Each was selected because of special facilities and personnel. The division frequently had to lend its aid toward assuring sufficient personnel, as by requesting Selective Service deferments, and also had to obtain priorities for materials. Such matters were promptly handled by Division personnel, and for the entire war period no contractor suffered seriously from lack of either men or materials.

Not a little of the division's success is to be credited to the conscientious exertions of its contractors. Even the few projects which did not produce immediately usable results for gun improvement nevertheless added to technical knowledge. They also frequently developed new techniques, processes, or materials which will have valuable peacetime applications.

The division established an early let-alone policy that produced excellent results from its contractors — academic, commercial, and professional. This policy permitted every contractor to solve his assigned problems in his own way, under general direction only, in keeping with established OSRD Policy.

## No-Profit-No-Loss

All but two of Division 1's thirty-six nongovernmental contracts, primarily for research and development, were operated on the unique no-profit-no-loss policy of OSRD.[1] Seventeen of the thirty-eight contracts under the division's supervision were made with academic or privately endowed research organizations, and the balance with commercial firms.

An outstanding development of this war research was the new spirit of cooperation which quickly grew up between former competitors. Intense business rivalry between some such concerns in peacetime had naturally extended into their research laboratories. But when the occasion demanded, as the gun erosion program got underway, representatives of diverse organizations all sat down together, "took their hair down," cheerfully revealed secret practices and technical details, without essential reservations and with little thought as to the effect on future profits.

[1]The two exceptions were the Crane Company's Contract OEMsr-1414, calling for preparation of a specific number of Stellite-lined machine-gun barrels; and the Midvale Company's OEMsr-1499, also a manufacturing contract.

## SOME WHO HELPED DO THE JOB

During late 1945 every contractor of Division 1 was invited to submit the names of the technical personnel "who had contributed in any material way to the advancement of the program."

In this appendix these names are listed as submitted for each contractor. The list includes some who gave part-time services, and others who were employed for less than the full period covered by the contract. The name of the directing head or heads of the project appears at the top of the list.

These men and women who stand out among the millions in the "Army behind the Army," are entitled to gratitude and appreciation, as are the hundreds of others who aided on the job, full-time or part-time, whether in the laboratories, shops, drafting rooms or offices of the contractors.

Division 1 desires to close its books with the acknowledgment that it found their efforts untiring, their resourcefulness often amazing, their contributions beyond adequate evaluation in the herculean task that carried the Nation and its Allies to victory.

## CHRONOLOGICAL SUMMARY OF CONTRACTS [2]

### *1941*
### *Carnegie Institution of Washington*
### *Geophysical Laboratory, Washington, D.C.*
### *Contract OEMsr-51*

Contract OEMsr-51 with the Carnegie Institution of Washington was the first contract of what later came to be known as Division 1, NDRC. It was initiated on July 15, 1941, for experimental investigations at the Geophysical Laboratory in Washington, D.C. The subject work clause of the contract was broadly expressed to cover studies of the erosion of guns and other ordnance problems. Much of the work on the mechanism of gun erosion was performed at the Geophysical Laboratory and even after other contractors had been found by the Division to take care of particular phases of this work, the Geophysical Laboratory acted as a "clearing house" to co-ordinate the information on this subject.

Later these results were applied to improvements in machine-gun barrels, first through the use of chromium plate on the bore surface and later through the use of such chromium plate in combination with the Stellite breech liner that had been developed by the Crane Company and the Haynes Stellite Company, two other contractors of the Division. The results on the mechanism of gun erosion were also applied to the problem of the search for erosion-resistant materials that might be inserted as liners in gun barrels.

Extensive work in the theory of interior ballistics was undertaken, at first because of its application to the problem of gun erosion; later it was extended to

[2]These summaries were prepared for this appendix by Dr. J. S. Burlew.

The chronological arrangement is broken in some cases where contractors held more than one contract; that is, all contracts are described under one company heading.

An alphabetical reference list of contractors will be found at the end of this appendix.

embrace the more general problem of hypervelocity guns. The initial planning and some preliminary experiments both on tapered-bore guns and sabot projectiles were carried out at the Geophysical Laboratory. The building at the Geophysical Laboratory was the location of the office of Section A–A (Division 1) from December 1941 until October 1944. After the latter date, when the operating office of the Division was moved to Philadelphia, the Geophysical Laboratory continued to be the headquarters of the Division Chief. By the time this contract had been terminated on December 31, 1945, a total of about $750,000 had been spent, which made it the largest of the Division's contract commitments. This amount did not include the salaries of regular staff members of the laboratory, or any overhead allowance, which were contributed by the Carnegie Institution.

L. H. Adams, Director
N. L. Bowen, Co-ordinator of Research

| | |
|---|---|
| Allen, Elizabeth | Laboratory Assistant |
| Bartram, H. W. | Assistant Physicist |
| Benedict, W. S. | Physical Chemist |
| Black, Walter F. | Laboratory Helper |
| Brooks, H. T. | Consulting Physicist |
| Brown, P. R. | Laboratory Assistant |
| Burlew, J. S. | Physical Chemist |
| Cail, D. E. | Radio Technician |
| Caldwell, E. T. | Laboratory Assistant |
| Comenetz, George | Mathematical Physicist |
| Cresson, G. V. | Statistician |
| Critchfield, C. L. | Mathematical Physicist |
| Crocker, J. Allen | Radio Engineer |
| Curtiss, C. F. | Assistant Physical Chemist |
| Davis, G. L. | Physicist |
| Dumas, M. E. | Laboratory Aide |
| Enagonio, Delmo | Assistant Chemist |
| England, J. L. | Assistant Physicist |
| Flannery, M. A. | Radio Engineer |
| Frankel, Elaine | Assistant Mathematician |
| Garten, William, Jr. | Physicist |
| Gibson, R. E. | Physical Chemist |
| Goff, R. H. | Assistant Physicist |
| Goranson, R. W. | Petrologist |
| Greig, J. W. | Petrologist |
| Hannan, P. J. | Laboratory Assistant |
| Heaston, J. R. | Laboratory Assistant |
| Heyl, P. R. | Consulting Physicist |
| Hirschfelder, J. O. | Physical Chemist |
| Ingerson, Earl | Petrologist |
| Jensen, Einar | Assistant Chemist |
| Jerabek, H. S. | Metallurgist |
| Johnson, Mrs. Nancy L. | Assistant Mathematician |
| Johnson, Richard E. | Mathematician |
| Jost, John | Chief Mechanician |
| Karush, William | Mathematician |
| Kershner, R. B. | Mathematician |
| Kneen, O. H. | Technical Writer |
| Koehl, G. M. | Assistant Physicist |
| Kracek, F. C. | Physical Chemist |
| Kracek, Mrs. Opal F. | Assistant Mathematician |
| Lees, W. L. | Physicist |
| Levin, Alex | Laboratory Assistant |
| Loeffler, O. F. | Laboratory Assistant |
| MacQuaid, C. A. | Laboratory Assistant |
| Marsh, C. A. | Assistant Chemist |
| McClunin, O. R. | Instrument Maker |

| | | | |
|---|---|---|---|
| Merwin, H. E. | Petrologist | Smith, M. D. | Laboratory Assistant |
| Metzelaar, P. N. | Assistant Physicist | Smith, N. M. | Physicist |
| Millar, J. McG. | Assistant Physicist | Spitz, Hillel | Assistant Physicist |
| Newcomer, J. E. | Instrument Maker | Stevenson, K. C. | Laboratory Assistant |
| Osborne, E. F. | Petrologist | Stone, A. H. | Mathematical Physicist |
| Phair, George | Assistant Physical Chemist | Stonebraker, G. G. | Machinist |
| Piggot, C. S. | Physicist | Sullivan, Mannevillette | Laboratory Assistant |
| Posnjak, Eugene | Chemist | Sutton, P. P. | Physical Chemist |
| Ramisch, J. L. | Instrument Maker | Tunell, George | Petrologist |
| Rappolt, J. | Aide | Tunell, Mrs. Ruth P. | Assistant Physicist |
| Rasmussen, G. S. | Laboratory Assistant | Urry, W. D. | Physical Chemist |
| Roberts, H. S. | Physicist | Vollmer, M. R. | Instrument Maker |
| Rowe, F. A. | Instrument Maker | Wildman, H. T. | Instrument Maker |
| Salomon, E. F. | Instrument Maker | Wrench, J. W. | Assistant Mathematician |
| Schairer, J. F. | Physical Chemist | Wright, F. E. | Petrologist |
| Schwab, Verne | Laboratory Assistant | Young, J. P. | Laboratory Assistant |
| Severinghouse, Mrs. Helen C. | Scientific Aide | Zies, E. G. | Chemist |
| Seymour, L. A. | Laboratory Assistant | | |
| Shepherd, E. S. | Physical Chemist | | |
| Sherman, Jack | Physical Chemist | | |

## *1942*
## *Western Electric Company, New York, N.Y.*
## *Bell Telephone Laboratories, Murray Hill, New Jersey*
## *Contract OEMsr-430*

Contract OEMsr-430 was initiated on January 15, 1942, in order that the facilities for electron diffraction and X-ray diffraction at the Bell Telephone Laboratories might be made available to Division 1. These specialized techniques were applied in the examination of a wide variety of specimens, including some from eroded gun bores as well as many specially prepared for the purpose. This contractor always worked in close co-operation with the staff of the Geophysical Laboratory. The contract was terminated on October 31, 1945.

L. H. Germer, Research Physicist

| | | | |
|---|---|---|---|
| Greenfield, E. | Technical Assistant | Lander, J. J. | Member Technical Staff |
| Grieco, A. | Technical Assistant | Reitter, G. E. | Technical Assistant |
| Haworth, F. E. | Member Technical Staff | | |

### *Contract OEMsr-1184*

The early contract on electron diffraction of eroded gun-bore surfaces led in an indirect way to the initiation of an ambitious attempt to deposit an erosion-resistant material on the bore surface of a gun. One of the members of the staff of the Bell Telephone Laboratories, speculating on the possible ways of applying such materials, decided that it might be possible to employ a pyrolytic process by which some metals can be deposited in a vacuum from the vapor of the carbonyl of the metal. Preliminary experiments carried out under Contract OEMsr-430 gave promising results. Hence, a separate contract, OEMsr-1184, was initiated on July 1, 1943. By the time it had been terminated on November 30, 1945, experiments costing about $210,000 had led to the development of a workable process for applying a hard coating of molybdenum or of tungsten, or of an alloy of both to the surface of steel or other material. It turned out that the process is not suitable for use with gun bores, because heating of the bore surface during firing so changes the coating that it spalls from the surface. There is likelihood, however, that this process will have important industrial applications.

H. Fletcher, Director of Physical Research, Director of Project[3]
R. R. Williams, Chemical Director, Director of Project[4]

| | | | |
|---|---|---|---|
| Armstrong, Helen E. | Technical Assistant | Lander, J. J. | Member Technical Staff |
| Bouton, G. M. | Member Technical Staff | Larson, E. C. | Member Technical Staff |
| Fang, P. Y. | Member Technical Staff | Reitter, G. E. | Mechanic and Technical Assistant |
| Germer, L. H. | Research Physicist | Schumacher, E. E. | Research Metallurgist |
| Greenfield, Edith A. | Technical Assistant | Selker, M. L. | Member Technical Staff |
| Greiner, E. S. | Member Technical Staff | Stitzel, H. H. | Mechanic |
| Haworth, F. E. | Member Technical Staff | Teal, G. K. | Member Technical Staff |
| Kohman, G. T. | Research Chemist | Thomas, E. E. | Member Technical Staff |
| Laffoon, C. M., Jr. | Visiting Engineer | Thurber, E. A. | Member Technical Staff |

### *Johns Hopkins University, Baltimore, Md.*
### *Contract OEMsr-463*

Contract OEMsr-463 was started on April 15, 1942. It provided for studies of the effect on gun steel of carbon monoxide and other gases. This extensive investigation, which was carried out in close co-operation with the Geophysical Laboratory,

[3]July 1943 to April 1944; March to November 1945.
[4]May 1944 to February 1945.

lasted until October 31, 1944, when the contract was transferred to the Ordnance Department of the Army. The total allocation of funds was $99,500.

J. C. W. Frazer, Professor of Chemistry[5]
F. Hubbard Horn, Project Director[6]

| | | | |
|---|---|---|---|
| Evans, R. C. | Assistant Director | Shapiro, Z. M. | Research Chemist |
| Hubard, S. S. | Research Chemist | Wagner, R. L. | Research Chemist |

*Jones and Lamson Machine Company, Springfield, Vt.*
*Contract OEMsr-467*

Contract OEMsr-467 was started on April 15, 1942, for the design and construction of a tapered-bore gun. At the beginning of 1944 this contractor took over the work carried on up to then by the Bryant Chucking Grinder Company, also of Springfield, on the design and construction of deformable projectiles for the same gun. When the contract was closed on November 4, 1945, the project had involved a total expenditure of about $360,000, counting the money spent by the Bryant Chucking Grinder Company. The 57/40-mm. tapered-bore gun that was developed was turned over to the Ordnance Department for test.

G. G. Leitch, Manager of Research
E. L. Rose, Director of Research

| | | | |
|---|---|---|---|
| Batchelder, J. W. | Research Engineer | Jones, E. R. | Research Engineer |
| Boyce, M. H. | Research Assistant | Lovely, J. E. | Vice-President |
| Carney, K. G. | Research Assistant | Ringie, D. F. | Development Engineer |
| Eldridge, F. R. | Physicist | | |

*Leeds and Northrup Company, Philadelphia, Pa.*
*Contract OEMsr-536*

Contract OEMsr-536, begun on April 15, 1942, had as its purpose the development of methods and equipment for determining the temperature of the bore surface of a gun barrel. A suitable technique for measuring the heat input to the bore surface was developed, by means of which data were obtained for use in calculations of temperature of that bore surface. Also, in co-operation with the Franklin Institute, this contractor carried out experiments on the effects of propellant composition on the heat input. The measurement of that quantity was shown to be a quick means of evaluating the erosiveness of a propellant. This contract was continued until November 30, 1945.

R. C. Machler, Chief of Physical Division Research Department
I. Melville Stein, Vice-President and Director of Research

| | | | |
|---|---|---|---|
| Armi, E. L. | Research Technologist | Polster, N. E. | Research Assistant |
| Johnson, J. L. | Research Technologist | | |

[5]Deceased July 1944.
[6]After July 1944.

## *Johnson Automatics, Inc., Boston, Mass.*
## *Contracts OEMsr-465,-746,-1433*

Preliminary tests of erosion-resistant liners and coatings in caliber .30 barrels were started on May 1, 1942, under Contract OEMsr-465. This contract was terminated after a little more than a year, because by that time adequate facilities for more extensive tests of this type in caliber .50 barrels had been set up at the Franklin Institute.

Contract OEMsr-746 had as its objective the design, construction and testing of an improved 20-mm. automatic cannon. During the period from October 8, 1942, when this contract was initiated, to June 30, 1945, when the work was terminated, two complete guns of different models had been designed, built and tested, at a total cost of $268,000. Although all difficulties had not been solved, the results were sufficiently encouraging that later the Navy Department took over the second finished gun for experimental use in a hypervelocity project by one of its contractors.

A request from the Army for the insertion of Stellite liners in caliber .30 barrels led to the initiation of two contracts for this purpose on December 8, 1944, one of which was the Johnson Contract OEMsr-1433. The small quantity of these lined barrels produced under this contract were turned over to the Ordnance Department for testing.

Melvin M. Johnson, Jr., President and Technical Director

Anderson, Frank — Production Machine Operator
Arbour, John — Tool Maker
Barnes, Robert — Production Machine Operator
Blair, Harry — Works Manager
Blease, William — Tool Maker
Brophy, James — Purchasing Agent
Butler, Wallace — Project Foreman
Casson, Wesley C. — Research and Development Engineer
Fox, George — Tool Maker
Gardiner, Chandler — Development Engineer
Hultenius, Gunnar — Tool Maker
Johnson, Richard — Supervisor, Barrel Room
Kerr, Daniel — Production Machine Operator
Lennon, Bernard — Tool Maker
Leveille, Pierre — Production Machine Operator
McKee, Robert — Design Engineer
O'Brien, George — Tool Maker
Pearson, Walter — Model Maker
Peterson, Axel — Production Machine Operator
Smith, Henry — Barrel Inspector
St. Amant, Horace — Chief Design Engineer
Thomas, Paul — Production Machine Operator
Valerien, Roland — Production Machine Operator
Wright, Daniel — Tool Engineer.

## *The Catholic University of America, Washington, D.C. Contract OEMsr-516*

On May 1, 1942, contract OEMsr-516 was arranged for calculations of stresses in the base of a high-explosive shell. This problem had its inception in a request from the Ballistics Research Laboratory, Aberdeen Proving Ground, made to NDRC the previous summer. That request had been assigned a separate War Department project number (OD-42) and for some time was carried along by the Division as an entirely separate venture. It was necessary to carry on extended experimental investigations about the nature of band pressure and the process of engraving. Later it was found that the results of this investigation were of considerable importance to the general planning of a well-rounded attack on the hypervelocity problem. Hence during 1944 it was possible to integrate the work under this contract with that of a number of the other contractors dealing with ballistics. Finally, on July 1, 1945, the scope of the contract was broadened to include a theoretical study of some fundamentals of interior ballistics. This phase of the work was continued under a Navy contract after Contract OEMsr-516 had been terminated on November 30, 1945.

Karl F. Herzfeld, Professor of Physics

| | | | |
|---|---|---|---|
| Biberstein, Frank A. | Research Engineer | Kracek, Mrs. Opal | Computer |
| Brown, Ralph E. | Instrument Maker | Krafft, Joseph M. | Research Engineer |
| Cattaneo, Louis E. | Research Engineer | McBrien, Vincent O. | Mathematician |
| Griffing, Virginia | Mathematician | Whelan, William T. | Research Engineer |
| Johnson, R. E. | Mathematician | Wrench, John W., Jr. | Mathematician |
| Jordan, Henri A. | Mathematician | | |

## *Franklin Institute, Philadelphia, Pa. Contract OEMsr-533*

During June of 1942 five additional contractors were drawn into the fold. Contract OEMsr-533, with Franklin Institute, initiated on June 1, 1942, originally was for the purpose of testing the erosion resistance of various materials in the form of short liners in the breech end of a hypervelocity caliber .50 erosion-testing gun, designed and constructed for the purpose. The important results obtained with this testing device and its general applicability to other problems led to a gradual expansion of the facilities at the Franklin Institute, until by the end of 1943 this contractor had been designated as another "central laboratory" of the Division, serving similarly to the Geophysical Laboratory as a clearing house for various activities. By the middle of 1944 some of the devices developed at the Franklin Institute had progressed to the stage where relatively large-scale engineering projects had evolved from them. This change involved a corresponding increase in expenditures with much of the work being done under subcontracts

and purchase orders. Furthermore, beginning on October 1, 1944 this contractor provided office space and some personnel for the Division 1 office. By the time the contract was terminated on February 28, 1946, the expenditures totaled approximately $510,000, which made it the second largest of the Division's contract commitments.

Nicol H. Smith, Associate Director

| | | | |
|---|---|---|---|
| Argue, John | Technician | Pearson, Forrest | Technician |
| Arnold, I. | Machinist | Pray, Thurman | Technician |
| Barnes, D. | Machinist | Riegel, Elizabeth | Technician |
| Barnes, James | Physicist | Ross, Mrs. Pearl | Technician |
| Blunt, C. | Machinist | Rust, Philip G. | Engineer |
| Casselman, Paul | Technician | Saccas, George | Draftsman |
| Charbonneau, George | Technician | Schaeffer, Olive | Technician |
| Collins, Mrs. Dorothy | Technician | Schiwall, N. | Machinist |
| Coulbourne, H. | Machinist | Schwartzman, G. H. | Machinist |
| Duffield, A. R. | Machinist | Schweiger, F. A. | Machinist |
| Edgar, John | Metallurgist | Shellington, W. H. | Machinist |
| Fulcher, Gordon | Physicist | Simons, Euretta | Technician |
| Gay, C. M. | Technician | Simpson, F. R. | Engineer |
| Gurnee, D. C. | Draftsman | Smith, G. | Machinist |
| Henninger, George W. | Engineer | Speakman, Dorothea | Technician |
| Hill, G. | Machinist | Streletz, Leo | Machinist |
| Hoyt, John E. | Physicist | Thorpe, James | Machinist |
| Jenni, Norma | Technician | Waring, A. | Machinist |
| Palmer, F. | Physicist | Weitort, John | Technician |

### *Harvard University, Cambridge, Mass.*
### *Contract OEMsr-537*

Contract OEMsr-537 was initiated on June 1, 1942, in order that Division 1 might have the use of the metallographic laboratory at Harvard University. At first this contractor worked closely with Johnson Automatics on erosion tests and later with the Franklin Institute in its extensive program of testing both powders and bore-surface materials. The wide range of specimens tested made possible an important generalization concerning the relation between heat transfer to the bore surface and gun erosion which was contained in the final report that was submitted at the termination of the contract on November 30, 1945.

C. Harold Berry, Professor of Mechanical Engineering
John N. Hobstetter, Technical Director

| | | | |
|---|---|---|---|
| Allen, Richard E. D., Jr. | Technician | Brawn, Charles E. | Technician |

| | | | |
|---|---|---|---|
| Brooks, Marjorie G. | Technician | O'Connor, Margaret B. | Technician |
| Jameson, A. Gregory | Chief Metallographer | Pearsall, Jeanne T. | Administrative and Technical Assistant |
| Key, Anna E. | Chief Technician | | |

### *Bryant Chucking Grinder Company, Springfield, Vt.* *Contract OEMsr-534*

On June 1, 1942, two contracts were inaugurated, both dealing with deformable projectiles for tapered-bore guns. Contract OEMsr-534 was a companion contract to that previously arranged with the Jones & Lamson Company. Bryant's job was to design and construct the special projectiles that were to be fired from the tapered-bore gun being designed and made by the other company. The two of them worked hand in hand until December 31, 1943, when this contract was terminated. After that the Jones & Lamson Company continued the project alone with the successful results already indicated.

Edwin L. Rose, Research Director

| | | | |
|---|---|---|---|
| Ansart, Arthur | Draftsman | Thoms, David B. | Design and Testing Engineer |
| Grobety, Paul Andre | Research Administrator | Vanderweil, Raimund Gerhard | Engineer |

### *University of Virginia, Charlottesville, Va.* *Contract OEMsr-598*

Contract OEMsr-598 was arranged on June 1, 1942, in order that the ultracentrifuge at the University of Virginia might be used for spinning tests to help the designers of deformable projectiles. During the year this contract was in force a number of projectiles prepared by the Bryant Chucking Grinder Company were tested.

J. W. Beams, Professor of Physics

| | | | |
|---|---|---|---|
| Podtiaguine, Michael P. | Research Associate | Snoddy, L. B. | Professor of Physics |

### *Crane Company, Chicago, Illinois* *Contract OEMsr-629*

Contract OEMsr-629, initiated on June 15, 1942, at first had as its objectives the design and construction of a replaceable steel liner for medium-caliber guns. Later the scope of the contract was enlarged to include liners of all types, with emphasis on erosion-resistant ones. As a result, the Crane Company played the most important part in the development of Stellite liners for machine-gun barrels. After a firing range had been installed there in 1944, tests were made of caliber .50 liners of various alloys, many of which were supplied by other contractors. This contractor thus became the focal point for the resistant-materials program during 1945. By the time that the contract had been closed on October 31, 1945, a

total of about $410,000 had been spent which made it the third largest of those supervised by Division 1.

Jules E. Stark, Manager, Engineering and Research Division
John P. Magos, Director of Research

| | | | |
|---|---|---|---|
| Cotterman, Frank | Research Engineer | Mueller, Alfred | Superintendent of Tool and Machine Design |
| Goldsmith, James R. | Metallurgical Engineer | Mueller, Raymond A. | Welding Engineer |
| Hard, Carl H. | Tool and Die Maker | Olsen, Arthur M. | Tool and Die Maker |
| Hedberg, Albert F. | Designing Engineer | Pierce, Joseph D. | Research Engineer |
| Hjulian, Julius A. | Tool and Die Maker | Stoyke, Ludwig | Testing Engineer |
| Kanter, Jerome J. | Materials Research Engineer | Sullivan, Regina | Laboratory Technician |
| Lange, Joseph O. | Patent Engineer | Thomson, George W. | Testing Engineer |
| Lauf, Herbert C. | Tool and Die Maker | Towner, Charles E. | Research Engineer |
| Lax, Frank A. | Designer | Ziegler, Nicholas A. | Research Metallurgist |
| Levey, John M. | Testing Engineer | | |
| Meinhart, Wilburt L. | Assistant Research Metallurgist | | |

*Contract OEMsr-1414*

This contract was initiated on September 1, 1944, as an off-shoot of the Crane Company's earlier contract. It provided for the preparation of 2,000 Stellite-lined caliber .50 machine-gun barrels; these were completed by the time the contract was terminated on February 28, 1945, at a total cost of $141,680. This contract was the "pay-off" of the Division's liner program, for these 2,000 barrels were sent by the Army and Navy into combat areas in different parts of the world so that they might be tested under actual fighting conditions. The results of these tests were so successful that the War Department then entered into a number of production contracts for thousands of such barrels.

Vincent P. Rumely, Vice-President — Manufacturing Division
G. C. Detlefsen, Works Manager
John P. Magos, Director of Research

| | | | |
|---|---|---|---|
| Cotterman, F. D. | Research Engineer | Mueller, A. | Superintendent of Tool and Machine Design |
| Hedberg, Albert F. | Designing Engineer | Mueller, Raymond A. | Welding Engineer |
| Larson, George L. | General Superintendent | St. Germaine, Richard | Foreman |
| Lax, Frank A. | Designer | Schultz, Frank G. | Assistant Foreman |
| Levey, John M. | Testing Engineer | | |

### *Massachusetts Institute of Technology, Cambridge, Mass.*
### *Contract OEMsr-608*

Contract OEMsr-608, initiated on July 1, 1942, provided for the preparation of test specimens of various special materials in the Metallurgy Department. At first this contractor worked in close co-operation with the groups working under the Division's contracts with Johnson Automatics, Inc. and Harvard University. Later the scope was broadened and this contractor came to play an important part in the general program of the development of erosion-resistant materials.

John Wulff, Professor of Metallurgy

| | | | |
|---|---|---|---|
| Edwardsen, Theodore A. | Foreman and Chief Technician | Leschen, John G. | Research Engineer |

### *Armour Research Foundation, Chicago, Illinois*
### *Contract OEMsr-613*

Contract OEMsr-613, begun July 1, 1942, was for the purpose of subjecting a variety of steels and alloys to erosion tests by supersonic cavitation, using a technique recently developed there. These tests, which were completed by the time that the contract was terminated on June 30, 1943, helped give an insight into the process of the mechanism of erosion.

Thomas C. Poulter, Associate Director

| | | | |
|---|---|---|---|
| Betz, Howard T. | Physicist | Kurtz, Alton | Assistant Physicist |
| Leedy, Halden A. | Chairman, Physics Section | Ziegler, George E. | Scientific Adviser |

### *University of New Mexico, Albuquerque, New Mexico*
### *Contract OEMsr-668*

During the summer of 1942 some work on sabot projectiles was started at the Geophysical Laboratory. Then on August 10, 1942, Contract OEMsr-668 was initiated with the University of New Mexico for an intensified attack on this problem. By the time this contract was terminated on November 30, 1944, a total of $208,000 had been spent for the work there. It not only led to the development of several successful designs of sabot projectiles, but also furnished a broad view of the whole field of such projectiles.

E. J. Workman, Director of Projects
D. T. MacRoberts, Project Supervisor
C. E. Hablutzel, Project Supervisor

| | | | |
|---|---|---|---|
| Crozier, W. D. | Research Physicist | LaPaz, Lincoln | Research Mathematician |
| Dunlap, H. F. | Research Physicist | Serduke, J. T. | Research Assistant |
| Frantik, R. O. | Research Assistant | Viersen, S. K. | Research Assistant |

### *Westinghouse Electric and Manufacturing Company Research Laboratories, East Pittsburgh, Pa. Contract OEMsr-915*

Contract OEMsr-915, started on August 15, 1942, was for the purpose of preparing and testing samples of specially treated metals and alloys. At first, the work was limited to an attempt to develop ductile chromium. Later it was expanded to include work on hot-hard alloys, including Stellite and Refractalloy, in co-operation with several other contractors of the Division. By the time that the work at the laboratories was completed on March 31, 1946, a total of about $340,000 had been spent for this work. An important element in the operation was the model shop that had been set up in the summer of 1944 to care for the efficient preparation of the many liner assemblies needed for gun-firing tests.

P. H. Brace, Consulting Metallurgist

| | | | |
|---|---|---|---|
| Allen, N. A. | Student Engineer | Kiefer, W. B. | Tool Designer |
| Borkowski, Robert | Laboratory Technician | Laffoon, C. M., Jr. | Research Engineer |
| Bridger, H. J. | Research Engineer | Macha, E. A. | Research Engineer |
| Cameron, D. S. | Precision Grinding and Tool-Making | McCoy, Kenyon | Lathe Operator & Laboratory Tech. |
| | | McVicker, H. D. | Research Engineer |
| Edgar, J. K. | Research Engineer | Mosher, D. R. | Research Engineer |
| Farravo, Samuel | Lathe Operator | Schuff, Bernard | Laboratory Tech. |
| Gray, T. H. | Research Engineer | Tanner, O. G. | Tool Designer |
| Green, C. T. | Manufacturing Engineer | Thall, Francis | Laboratory Aide |
| | | Travis, Thomas | Tool and Die Maker |
| Gulbransen, E. A. | Research Engineer | | |
| Hawkins, C. T. | Laboratory Aide | Vanguro, Albert | Tool Designer |
| Houck, Paul | Laboratory Aide | Wahl, A. M. | Research Engineer |

### *Westinghouse Lamp Division, Bloomfield, New Jersey Contract OEMsr-1205*

By the spring of 1943 the efforts of the Westinghouse Research Laboratories to make ductile chromium had proved unsuccessful. Attention was then turned to the possibility of fabricating a hardened erosion-resistant alloy in a form suitable for use as gun liners. In order to be able to concentrate on it the best resources available, a separate contract was arranged on June 1, 1943, with another branch of the Westinghouse Electric Corporation contract OEMsr-1205, its Lamp Division at Bloomfield, New Jersey. By the time this contract was terminated on October 31, 1945, the initial development of a process of fabricating a new alloy (referred to as "Alloy X" in this history) had been completed and this alloy had been shown to give superior results as a liner for a hypervelocity gun. The Bureau of Ordnance of the Navy Department continued the project after that date under a contract with the Westinghouse Electric Corporation.

J. W. Marden, Assistant Director of Research

| | | | |
|---|---|---|---|
| Fraser, W. | Factory Foreman, Metals | Offinger, M. W. | Engineer |
| Gordon, J. | Factory Foreman | Ramage, J. | Consultant |
| Gray, J. E. | Engineer | Scales, J. | Experimental operator |
| Grimm, H. | Technical Assistant | Sherwood, Mrs. W. K. Jr. | Laboratory Assistant |
| Hall, R. D. | Consultant | Slavinski, Mrs. (Haas) | Laboratory Assistant |
| MacLachlan, J. | Technical Assistant | Wroughton, D. M. | Engineer |
| Newman, W. | Experimental operator | | |

## *National Bureau of Standards, Washington, D.C.*
## *Inductance and Capacitance Section*

Research facilities at the National Bureau of Standards at Washington, D.C., were utilized by Division 1. Although of course no contract was arranged, because this was another Federal Agency, transfers of funds from OSRD to the Bureau were made for specific projects. The first of these transfers, effective on September 1, 1942, initiated an investigation of bore friction and other features of interior ballistics. This work eventually led to the establishment of a small firing range on the grounds of the Navy's David Taylor Model Basin at Carderock, Maryland, near Washington, D.C. Important additions to the knowledge of the fundamentals of interior ballistics were obtained there. When Division 1 terminated its sponsorship at the end of October 1945, the Navy Department made a transfer of funds to the National Bureau of Standards for continuation of the project.

Harvey L. Curtis, Physicist

| | | | |
|---|---|---|---|
| Baker, Harriet | Aide | Kent, Genevieve | Aide |
| | | Libbey, Kathleen | Aide |
| Bennett, E. G. | Engineer | McDonald, Mary | Aide |
| Brooks, H. B. | Physicist | Moon, Charles | Physicist |
| Cahill, Margaret | Draftsman | Morrisson, Elizabeth H. | Aide |
| Carter, Mary F. | Draftsman | | |
| Cooter, I. L. | Physicist | Myers, R. D. | Physicist |
| Diehl, Zulime | Aide | Norris, Edith | Aide |
| Driscoll, Raymond L. | Physicist | Potter, Jane | Aide |
| | | Powell, Harriet | Aide |
| Follin, Elizabeth | Aide | Pritchett, Patricia | Aide |
| Griggs, Ruth | Aide | Pyle, Anne | Aide |
| Gross, Francis J. | Physicist | Rolf, Pauline | Physicist |
| Herman, Felice | Aide | Rossini, F. D. | Chemist |
| Herrington, E. P. | Navy Gunner | Ruegg, Churchill | Aide |
| Jansson, Alf D. | Draftsman | Scott, A. H. | Physicist |
| Jones, Phyllis | Aide | Snow, C. | Mathematician |

| | | | |
|---|---|---|---|
| Triplett, Merrill | Aide | Wheatley, Joan | Aide |
| Twigg, Rosalyn | Aide | White, Helen | Aide |

## *National Bureau of Standards*
## *Electrochemical Section*

A separate project at the National Bureau of Standards involved studies of improved chromium plate for gun barrels. This work had started on an informal basis during the summer of 1942 through co-operation with the Geophysical Laboratory. Then on October 1 of that year the first of a series of transfers of funds from OSRD was arranged which by 1945 had totaled $94,000. This work, which had led to a notable improvement in machine-gun barrel life, also was continued by the Services—in this case jointly by the Army and Navy—after Division 1 terminated its sponsorship on October 31, 1945.

William Blum, Principal Chemist

| | | | |
|---|---|---|---|
| Aberg, Wm. | Scientific Aide | Lilya, Donald | Scientific Aide |
| Brenner, Abner | Full Chemist | Metzger, Wm. H., Jr. | Junior Chemist |
| Burel, Louise | Scientific Aide | Riddell, Grace | Assistant Chemist |
| Burkhead, Mrs. Polly | Assistant Chemist | Seegmiller, Mrs. Emma Tubbs | Scientific Aide |
| Crafts, Mrs. Ruth M. | Junior Chemist | Seegmiller, Robert | Assistant Chemist |
| Crosthwaite, Richard | Scientific Aide | Sentel, Clara B. | Assistant Chemist |
| Hess, George F. | Scientific Aide | Siegel, Sidney | Scientific Aide |
| Hinton, James L. | Scientific Aide | Stigall, Eugene M. | Scientific Aide |
| Kellogg, Gene | Junior Chemist | Stuart, Mrs. Christine | Junior Chemist |
| Keyser, George | Scientific Aide | Tidler, J. Wilbur | Junior Chemist |
| King, Mrs. Alice O. | Junior Chemist | Wilson, Donald | Scientific Aide |
| Lamb, Vernon A. | Full Chemist | | |

## *Duke University, Durham, N.C.*
## *Contract OEMsr-733*

Contract OEMsr-733 was for the purpose of studying the burning rate of propellants. It was begun on October 1, 1942, and after Division 1 had gained as much information as it needed, the contract was transferred on January 24, 1944, to Section H of Division 3, NDRC.

L. C. Bonner, Assistant Professor of Physics

| | | | |
|---|---|---|---|
| Hendricks, W. A. | Technical Assistant | Trippe, D. F. | Technical Assistant |
| Hobbs, M. E. | Research Associate | Mouzon, J. C. | Research Associate |

*Contract OEMsr-1038*

Contract OEMsr-1038 was started on June 1, 1943, to provide for a theoretical investigation of the thermal effects in gun erosion. A thorough study of the physical problems of heat transfer and of the erosion vents was made. The procedure of calculation described in the elaborate report prepared during the five months of this contract served as a means of co-ordinating a vast amount of information on this subject gained from subsequent experiments.

L. W. Nordheim, Professor of Physics

| | | | |
|---|---|---|---|
| Nordheim, G. P. | Research Associate | Soodak, Harry | Research Associate |
| Scheraga, Miriam K. | Calculator | | |

*General Electric Company*
*Lamp Department, Nela Park, Cleveland, Ohio*
*Contract OEMsr-865*

The urge for an intensified drive on the development of erosion-resistant materials, in the fall of 1942, led to the initiation of Contract OEMsr-865 for the preparation of samples of erosion-resistant materials. Shortly after the contract had been arranged, however, the contractor found that other high-priority war jobs prevented the application of as much effort to this problem as originally planned. Hence, only about 10 per cent of the original appropriation of $54,000 was actually expended. The contract was continued in force, however, until May 22, 1945, because this contractor made available to the Geophysical Laboratory certain special alloys for erosion testing from time to time.

W. P. Sykes, Metallurgical Engineer

.....................................................................

*1943*

*Arthur D. Little, Inc., Cambridge, Mass.*
*Contract OEMsr-886*

During 1943 four new contracts were negotiated. The first of these was OEMsr-886 which was started on February 9, 1943, for experiments on the molding of plastics or other lightweight material into sabots for subcaliber projectiles. such as those being designed by the University of New Mexico under its contract. The two contractors worked in close co-operation during the 14 months that the contract lasted.

Max Knobel, Physicist
Thorne L. Wheeler, Vice-President

| | | | |
|---|---|---|---|
| Harford, Charles G. | Chemical Engineer | Packard, Robert H. | Electrical Engineer |

The other three contracts awarded in 1943 have already been described, each one being a second contract with a contractor already employed on Division 1 work. Thus OEMsr-1038, dated June 1, was a new contract with Duke University. OEMsr-1205, dated June 1, was with Westinghouse Lamp Division, Bloomfield, New Jersey. OEMsr-1184, dated July 1, was the second contract held by Western Electric Company, an outgrowth of developments made under the earlier contract.

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## *1944*

The year 1944 saw the initiation of the last of the Division's research contracts and the beginning of a series of development engineering contracts for making sufficient numbers of improved devices of warfare for extensive testing under combat conditions. The five research contracts dealt with the development of resistant materials of one kind or another. One of the engineering contracts was for the development of sabot projectiles, the other four[7] were for the production of small lots of Stellite-lined machine-gun barrels for tests under combat conditions.

### *Climax Molybdenum Company, New York, N.Y.*
### *Contract OEMsr-1273*

Contract OEMsr-1273 was initiated on February 1, 1944, in order to have this company's research laboratory in Detroit, Michigan investigate the applicability to gun liners of a special chromium-base alloy that had been developed in the course of research on materials for turbine blades. This contract also made it possible to see whether either of two new methods of melting molybdenum might have application to the gun liner problem. Before the contract was terminated on August 31, 1945, the chromium-base alloy had been shown to have considerable promise for use as a gun liner. Accordingly, arrangements had been made to have the Union Carbide and Carbon Research Laboratories undertake its development on a semicommercial scale. One of the methods of melting molybdenum, which involved the use of the thermite process, did not turn out to be successful; but the other process, by which the molybdenum is melted in a vacuum, seems very promising, although there are still some unsolved problems. Only $40,000 was spent under this contract because the Climax Molybdenum Company contributed without charge the services of the members of its laboratory staff.

A. J. Herzig, Vice-President
R. M. Parke, Director of Research

| | | | |
|---|---|---|---|
| Bens, Frederick P. | Metallurgist and Research Supervisor | Blackett, Harold H. | Technician |

[7]Two of these contracts have been described earlier in this appendix — Contract OEMsr-1433 with Johnson Automatics, Inc., on p. 430; and Contract OEMsr-1414 with Crane Company on p. 434.

| | | | |
|---|---|---|---|
| Blanchard, James R. | Metallurgist | Semchyshen, Marion | Metallurgist |
| Ham, John L. | Research Metallurgist | Stine, Wilson W. | Technician |
| Leavenworth, E. Kendrick | Mechanical Engineer | Timmons, George A. | Metallurgist and Research Supervisor |
| Maurer, Robert H. | Chemist | Trapp, Harvey E. | Assistant Chemist |
| Miller, John D. | Technician | Wadson, Gerald C. | Metallurgist |
| Pierce, Ethel C. | Librarian | | |

### *Contract OEMsr-1320*

Contract OEMsr-1320 was started on April 1, 1944, for the purpose of developing a method of preparing molybdenum carbonyl on a semicommercial scale, so that if the carbonyl process of plating molybdenum being developed by the Bell Telephone Laboratories became successful, a source of raw material would be available. Such a process had been developed by the time the contract was terminated on June 30, 1945.

Arthur Linz, Vice-President

| | | | |
|---|---|---|---|
| Coffer, L. Wallace | Chemist | Hamill, Hugh | Chemist |
| Errett, D. J. | Chemist | McCoy, A. L. | Chemist |
| Grimes, G. R. | Chemist | | |

### *Yale University, New Haven, Connecticut*
### *Contract OEMsr-1318*

Contract OEMsr-1318 was begun on March 15, 1944, for the purpose of having its Chemistry Department prepare chromium carbonyl, a very rare chemical, so that it could be used by the Bell Telephone Laboratories in some chromium plating experiments similar to those with molybdenum carbonyl. Later it was decided to have Yale also try the plating experiments. A satisfactory method for preparing chromium carbonyl was developed and preliminary plating experiments were carried out. Chromium deposited in this way is extremely hard and may have application in industry.

Arthur J. Hill, Professor of Chemistry
Benton B. Owen, Associate Professor of Chemistry

| | | | |
|---|---|---|---|
| Cassidy, Harold G. | Assistant Professor of Chemistry | Trust, H. Knowlton | Graduate Student of Chemistry |
| English, James, Jr. | Assistant Professor of Chemistry | Vanderbilt, Clarissa A. | Graduate Student of Chemistry |
| | | Webber, Robert T. | Instructor of Physics |

*Union Carbide and Carbon Research Laboratories, Inc., New York, N.Y.*
*Contract OEMsr-1330*

Contract OEMsr-1330, begun on May 1, 1944, covered the general subject of the development of hot-hard alloys having properties suitable for liners of gun barrels and the fabrication of such alloys in the form of gun liners or coatings. The first specific assignment involved the improvement of Stellite, a preliminary test of which at the Crane Company had already shown to be very promising material for gun liners. Subsequently, under this contract, the Haynes Stellite Company, another subsidiary of the Union Carbide and Carbon Corporation, prepared a great many Stellite liners which were tested by the Crane Company. The Research Laboratories in addition made a thorough investigation, in cooperation with other contractors of Division 1, of the physical properties of Stellite. In the spring of 1945 a separate assignment was made under this contract, namely, to provide for the development of methods of producing in commercial quantities the chromium-base alloys already given preliminary development by the Climax Molybdenum Company. At the termination of the OSRD contract on October 31, 1945, this phase of the work was continued under a contract with Army Ordnance Department.

J. H. Critchett, Vice-President
A. B. Kinzel, Vice-President
W. A. Wissler, Metallurgist and Project Director

| | | | |
|---|---|---|---|
| Bagley, G. D. | Chief Engineer | Forgeng, W. D. | Metallurgist |
| Cormack, C. E. | Metallurgist | Malman, I. S. | Metallurgist |
| Field, B. E. | Manager | Spendelow, H. R., Jr. | Metallurgist |
| Fowler, R. M. | Metallurgist | Wendt, H. F. | Chemist |

*Haynes Stellite Company*

E. E. LeVan, President
F. S. Badger, Vice-President
F. C. Kroft, Jr., Metallurgist

*Remington Arms Company, Bridgeport, Conn.*
*Contract OEMsr-1368*

Contract OEMsr-1368 entered into on May 9, 1944, was the first of the engineering contracts. This contractor was assigned the task of developing a production technique for a sabot projectile following the design already developed by the University of New Mexico, under Contract OEMsr-668. Hand in hand with the work on the pilot plant went further improvement of the design itself, following field tests made first at Aberdeen Proving Ground and later at a private range at Pine Camp, N.Y., made available to the Remington Arms Company by the Army. By the time the contract was terminated on October 15, 1945, a successful design of 90-mm. sabot projectile having a tungsten carbide core had been

developed, and specifications for a pilot plant to manufacture such projectiles were presented in the final report. This project involved the expenditure of about $270,000.

R. A. A. Hentschel, Engineering Superintendent, Technical Department

| | | | |
|---|---|---|---|
| Hammond, John | Group Leader, Production Engineering Unit | Kinraide, T. R. | Supervisor, Production Engineering Unit |
| Howell, J. D. | Design Engineer | Walker, M. H. | Design Engineer |
| Darby, Paul F. | Supervisor, Physics and Ballistics Unit | | |

*Contract OEMsr-1438*

Production Contract OEMsr-1438 was made December 8, 1944, for inserting Stellite liners in caliber .30 barrels. Small quantities were produced and turned over to the Army for tests.

R. A. A. Hentschel, Engineering Superintendent, Technical Department

| | | | |
|---|---|---|---|
| Benner, C. F. | Metallurgist | Smith, M. H. | Group Leader, Production Engineering Unit |
| Fontaine, K. B. | Process Engineer | | |
| Henriksen, P. F. | Process Engineer | | |
| Kinraide, T. R. | Supervisor, Production Engineering Unit | Wheat, E. K. | Group Leader, Production Engineering Unit |

*Industrial Research Laboratories, Los Angeles, Calif.*
*Contract OEMsr-1424*

The success of the Stellite liner brought with it an urge to explore other means of making such liners than by the method of investment casting used by the Haynes Stellite Company. Contract OEMsr-1424, started on August 1, 1944, provided for experiments in the preparation of such liners by means of centrifugal casting. By the time the contract was terminated on September 30, 1945, it had been shown that such liners could be made by this process with properties equivalent to those of liners made by investment casting; but there was still some question whether it was economically desirable to do so.

Walter F. Hirsch, Metallurgist

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## *1945*

*Chrome Gage Corporation, Philadelphia, Penna.*
*Contract OEMsr-1444*

The year 1945 saw the Division's roster of contractors completed by the addition of four more. Contract OEMsr-1444 was inaugurated on January 1, 1945, for

the purpose of setting up a pilot plant for the production of chromium-plated machine-gun barrels. This plant had just started production by V-J Day, when a "stop work" order was issued, because the Army Ordnance Department was canceling its orders for machine-gun barrels. The operations under this contract cost almost $400,000 provided by a transfer of funds from the War Department to OSRD.

M. G. Herbach, Treasurer
Frank J. Benoit, Superintendent of Operations

| | | | |
|---|---|---|---|
| Brown, Harry | Electroplater | Martin, S. M. | Chief Engineer |
| Campbell, J. M. | General Foreman | * Norman, Wm. Z. | Electrical Engineer |
| Ellis, William | Electroplater | Vezzosi, Thos. | Electroplater |
| Golder, Richard | Assistant Chemist | * Weisselberg, A. | Consulting Engineer |
| Haug, Gus, Jr. | Tool Designer | * Weisselberg, Hermann | Engineer |
| Haug, Gus, Sr. | Tool Designer | * Weisner, Leo | Physicist |
| Heiman, Samuel | Laboratory Electrochemist | | |

### *A. F. Holden Company, New Haven, Conn.*
### *Contract OEMsr-1473*

Two of the 1945 contracts were small ones covering special methods of insertion of gun liners in machine-gun barrels. Contract OEMsr-1473, started on Jan. 1, 1945, provided for experiments on the brazing of Stellite liners in machine-gun barrels. Only some preliminary experiments had been carried out by V-J Day, when the work was stopped.

Charles F. Hammond, Vice-President
George N. Vitt, Assistant to the President

### *Al-Fin Corporation, Jamaica, N.Y.*
### *Contract OEMsr-1494*

Contract OEMsr-1494, started on January 27, 1945, provided for the application of that Company's secret process of thermite welding to the problem of inserting Stellite on molybdenum gun liners. This contract was terminated in the fall of 1945, because of the cessation of hostilities, before the program outlined had been completed.

F. P. Culbert, Vice-President and General Manager

| | | | |
|---|---|---|---|
| Gray, Harold | Foreman in charge, Experimental and Foundry Work | Whitfield, M. G. | Consulting Engineer |
| Sheshunoff, V. | Consulting Engineer | Zapp, F. M. | Manager of Experimental and Development Work |

*Of A. Weisselberg, Consulting Engineers.

*Midvale Company, Philadelphia*
*Contract OEMsr-1499*

Contract OEMsr-1499 was started on June 1, 1945, for making some special 90-mm. gun tubes for a hypervelocity gun. This Company eventually completed two such tubes, which were turned over by Division 1 to the Army Ordnance Department in the spring of 1945 so that it could continue further development of the hypervelocity project started by Division 1.

F. B. Foley, Director of Research

| | | | |
|---|---|---|---|
| Reed Knox, Jr. | Research Assistant | Poppe, Isabella M. | Research Assistant |

## THE CONTRACTS OF DIVISION 1

| *Contractor* | *Contract No. OEMsr-* | *Effective Date* | *Termination Date* | *See Page* |
|---|---|---|---|---|
| Al-Fin Corporation | 1494 | 27 Jan. '45 | 31 Aug. '45 | 168 |
| Armour Research Foundation | 613 | 1 July '42 | 30 June '43 | 164 |
| Bryant Chucking Grinder Company | 534 | 1 June '42 | 31 Dec. '43 | 164 |
| Carnegie Institution of Washington (Geophysical Laboratory) | 51 | 15 July '41 | 31 Dec. '45 | 161 |
| Catholic University | 516 | 1 May '42 | 30 Nov. '45 | 163 |
| Chrome Gage Corporation | 1444 | 1 Jan. '45 | 15 Jan. '46 | 168 |
| Climax Molybdenum Company | 1273 | 1 Feb. '44 | 31 Aug. '45 | 166 |
| Climax Molybdenum Company | 1320 | 1 Apr. '44 | 30 June '45 | 167 |
| Crane Company | 629 | 15 June '42 | 31 Oct. '45 | 164 |
| Crane Company | 1414 | 1 Sept. '44 | 28 Feb. '45 | 164 |
| Duke University | 733[8] | 1 Oct. '42 | 24 Jan. '44 | 166 |
| Duke University | 1038 | 1 June '43 | 31 Oct. '43 | 166 |
| Franklin Institute | 533 | 1 June '42 | 28 Feb. '46 | 163 |
| General Electric Company | 865 | 1 Nov. '42 | 22 May '45 | 166 |
| Harvard University | 537 | 1 June '42 | 30 Nov. '45 | 163 |
| Haynes Stellite Company[10] | | | | |
| A. F. Holden Company | 1473 | 1 Jan. '45 | 30 Sept. '45 | 168 |
| Industrial Research Laboratories | 1424 | 1 Aug. '44 | 30 Sept. '45 | 167 |
| Johns Hopkins University | 463[9] | 15 Apr. '42 | 31 Oct. '44 | 162 |
| Johnson Automatics, Inc. | 465 | 1 May '42 | 30 June '43 | 162 |
| Johnson Automatics, Inc. | 746 | 8 Oct. '42 | 30 June '45 | 162 |
| Johnson Automatics, Inc. | 1433 | 8 Dec. '44 | 30 Sept. '45 | 162 |
| Jones & Lamson Machine Company | 467 | 15 Apr. '42 | 4 Nov. '45 | 162 |
| Leeds and Northrup Company | 536 | 15 Apr. '42 | 30 Nov. '45 | 162 |
| Arthur D. Little, Inc. | 886 | 9 Feb. '43 | 31 Mar. '44 | 166 |
| Mass. Institute of Technology | 608 | 1 July '42 | 30 Sept. '45 | 164 |
| Midvale Company | 1499 | 1 June '45 | 31 Mar. '46 | 168 |
| National Bureau of Standards Electrochemical Section | | 1 Oct. '42 | 31 Oct. '45 | 165 |
| National Bureau of Standards Inductance and Capacitance Section | | 1 Sept. '42 | 31 Oct. '45 | 165 |
| Remington Arms Company, Inc. | 1368 | 9 May '44 | 15 Oct. '45 | 167 |
| Remington Arms Company, Inc. | 1438 | 8 Dec. '44 | 15 Sept. '45 | 167 |

| *Contractor* | *Contract No. OEMsr-* | *Effective Date* | *Termination Date* | *See Page* |
|---|---|---|---|---|
| Union Carbide and Carbon Research Laboratories | 1330 | 1 May '44 | 31 Oct. '45 | 167 |
| University of New Mexico | 668 | 10 Aug. '42 | 30 Nov. '44 | 164 |
| University of Virginia | 598 | 1 June '42 | 31 May '43 | 164 |
| Western Electric Company (Bell Telephone Laboratories) | 430 | 15 Jan. '42 | 31 Oct. '45 | 162 |
| Western Electric Company (Bell Telephone Laboratories) | 1184 | 1 July '43 | 30 Nov. '45 | 162 |
| Westinghouse Lamp Division | 1205 | 1 June '43 | 31 Oct. '45 | 165 |
| Westinghouse Research Laboratories | 915 | 15 Aug. '42 | 31 Mar. '46 | 165 |
| Yale University | 1318 | 15 Mar. '44 | 30 Sept. '45 | 167 |

[8]Transferred to Division 3, NDRC.
[9]Transferred to Ordnance Department.
[10]See Union Carbide and Carbon Company, p. 167.

# APPENDIX 2

# *EROSION — CURRENT STATUS*[1]

The Nature of Erosion and Factors Affecting It:

The erosion of guns is a service wear phenomenon. There are two general types:

a) Surface scouring
b) Surface layer fragmentation

In guns of high rates of fire the surface scouring predominates. In other guns the surface layer fragmentation predominates but surface scouring is an important factor. The temperature of the gun is a determining factor correlating with the type of erosion. For guns worn at lower temperature erosion proceeds largely by surface cracks because of which sizeable particles of metal are torn from the surface. At high gun temperatures the removal of the surface proceeds at such a high rate that heat checks do not exert the same influence.

The metal of the gun bore is subject to complex physical changes, due to surface reactions of the metal with the products of combustion, to the diffusion of gaseous and probably metallic elements into the steel and to the application of high temperatures and high temperature gradients.

At the temperatures developed in the combustion of powder probably some gaseous elements are in the atomic stage. In such condition elements like oxygen readily unite with the metallic atoms in the surface, forming compounds which are easily torn from the surface by the stream of powder gases. Also atoms such as those of hydrogen, nitrogen, oxygen and carbon are readily diffused into steel, the rate of diffusion increasing as an exponential function of the temperature. An effect of this diffusion is the formation of a hard surface layer known as the white layer. The structural change in the metal proceeds much deeper than the surface layer. The composition and properties of the white layer are not understood. Some authorities have claimed to have identified nitrides, and others carbides. Probably a highly complicated structure exists. Except for hardness the physical properties and structure of this layer are unknown. It should be noted that this white layer also follows surface cracks and probably influences crack development. It may be beneficial in retarding erosion because of higher physical properties incidental to increased hardness. The fact that guns do not fail in fatigue in the presence of high tension stress concentrations around cracks in the bore surface may be due in part to the physical properties of the white layer. Exact information to substantiate this hypothesis does not exist.

[1]Prepared by Colonel F. G. Jenks at the request of General E. P. McFarland, Assistant to Chief of Ordnance, and sent to him on March 3, 1941.

The diffusion of metallic elements into the gun steel has not been investigated. Both copper and particularly zinc, which are used in rotating bands and jackets, diffuse in steel at the temperatures existing in the bore. Zinc has been a troublesome element in causing cracks in steel under conditions of comparatively high temperatures. Investigations of the behavior of zinc in steel have been made in connection with riveting galvanized plate and also in welding. Measurements made at the Proving Ground about ten years ago in determining the comparative behavior of copper and zinc-copper rotating bands indicated a higher erosion rate for the zinc-copper alloy. No investigation was made, however, to determine why.

Evidence has been seen which indicates the presence of intergranular penetration of some elements which probably influences the rate of growth of surface cracks. Insufficient work has been done, however, to understand the significance of this form of failure.

Heat checks or surface cracks are due to several causes among which are internal stresses induced by surface irregularities in the presence of high temperature gradients, internal stresses incidental to differences of coefficients of expansion of different compounds contained in steel and internal stresses incidental to volume changes in the metal cooling through critical temperatures. Heat checking is influenced by composition of the steel, size, distribution and composition of nonmetallic inclusions and the roughness of the surface finish.

The honing of bores will not make much difference as the critical heat checking is that at the bottom of the groove which is cut after honing. Considerable work has been carried on in industrial laboratories on heat checking experienced in other products.

It may be noted that heat checks penetrate at first normal to the surface. The crack as it contacts changes in structure such as those occasioned by nonmetallic inclusions is deflected laterally. As these cracks grow a surface particle is finally torn off by surface friction or by other causes. Heat checking is most serious at the origin of rifling.

Materials such as steel are subject to transformation of atomic structure. During this transformation volume changes occur which affect internal stresses. The temperature at which these changes occur depends upon composition. Alloys exert a great influence on the temperature at which this transformation takes place. It may be expected therefore that heat checking is sensitive to the alloy content of steel.

The strength of steel at elevated temperatures varies considerably according to the alloy composition. A series of steels of superior physical properties for high temperature service has been developed for public utilities and the chemical industries. It might be expected that steels of higher physical properties at elevated temperatures might be useful in reducing erosion wear. Insufficient data is available to judge the significance of these properties.

The rate of erosion is affected by the temperature of the inner surface layer and consequently by the rate of fire. At the higher temperatures wear by surface friction proceeds faster both due to increased rate of surface chemical reactions and decreased strength of material. The diffusion of gaseous elements is increased with temperature and possibly there is a change in the mechanism of penetration.

Heat checking is probably less due to decreases in temperature gradients. It is possible that the removal of the surface layer proceeds more rapidly when hard layers are built up by diffusion. No satisfactory data are available as to the physical phenomena involved. Plenty of statistical data is available as to the increased rate of wear at high temperatures.

Our specifications for nonmetallic inclusions in gun steel are based upon the effect of nonmetallics on transverse physical properties. In inspection practice the objections to nonmetallic segregations at bore surfaces have been considered. As such practice has not been incorporated in specifications there has been a trend toward neglecting the factor in the development of production methods. No exact information is available on correlation of nonmetallic inclusion and their composition, size and distribution with erosion.

Band and jacket compositions have been selected on the basis of rate of coppering rather than on effect on erosion. Few data are available as to the influence of band composition on erosion rate.

Powder composition and especially the temperature of combustion influences the heat input and the temperature of the gun wall. The decomposition products also probably influence erosion.

The design of the gun influences rate of erosion. Increased heat capacity by the use of heavy walls or by means of increasing the rate of heat dissipation from the gun decreases the rate of erosion. No engineering studies in this subject are known.

Factors Requiring Study:

1 — Gun
- a — Design as affecting heat capacity and heat dissipation
- b — Chamber design — turbulence of gas stream
- c — Bore finish
- d — Plating of bore
- e — Use of composite metals

2 — Metal
- a — Composition
  - (1) Strength at room and elevated temperatures
  - (2) Strength at high rates of loading
  - (3) Resistance to heat checking
  - (4) Critical temperatures on heating and cooling
  - (5) Nonmetallics; composition, size, frequency and distribution
  - (6) Conductivity
- b — Heat Treatment
  - (1) Metallographic structure
  - (2) Special bore hardening treatments
- c — Behavior in erosion under automatic fire and controlled rapid fire
  - (1) Changes in surface layer
  - (2) Changes in structure
  - (3) Identification of diffusion products
  - (4) Heat checking
  - (5) Rate of loss of metal from critical bore sections

3 — Projectile
  (1) Band composition
  (2) Effect of elements in band upon steel at elevated temperature
  (3) Frictional resistances
  (4) Bearing properties and characteristics
4 — Cartridge Case
  (1) Design
  (2) Composition of metal lost in firing
  (3) Heat dissipation
5 — Propellants
  (1) Composition and decomposition products
  (2) Combustion temperature
  (3) Pressure
  (4) Weight

Work Done and in Progress:

Some work was done during the past war in study of the white layer. It was reported as a nitrided product. Work carried on abroad since has reported the hard layer as a carburized product. Watertown Arsenal has inaugurated some preliminary microscopic work on the white layer but the amount of work accomplished has been so little that no real knowledge has been added. Some microscopic work has also been carried on by students at engineering schools. Those contributions have also been unimportant. No work in the investigation of the mechanism of erosion is known to be under way in this country at present. There have been important developments in laboratory instruments which, if applied to the erosion problem, should give us a far better understanding of the mechanism of erosion and the properties and characteristics of erosion products and structural changes. Particular reference is made to X-ray diffraction and other spectrographic methods, microchemical methods and improved microscopic apparatus. Physical metallurgy has advanced greatly during the past two decades and the experience and methods of that science are now available to a study of erosion.

The technique of physical erosion study has been developed in study of stress corrosion, intergranular corrosion and erosion of materials, etc. used by various industries and in study of heat checking under varied service conditions.

Immediately after the World War a large amount of statistical data on the wear of guns was collected from French, British and American sources. So far as known no attempt has been made to correlate that data with production and design data. There is also a large amount of statistical data available from Proving Ground and Springfield Armory records. Correlation of such data with fundamental factors is difficult. So far as known no real study of this mass of data has been made.

A change was made in the composition of steels used in small arms barrels (from 1350 to 4150). The steel now used has better properties at elevated temperatures and is understood to possess better resistance to erosion. Some steels with better high temperature properties than 4150 have been fired at Springfield Armory and unfavorable reports have been submitted. The reports of test avail-

able are too inadequate to warrant a conclusion as to whether the test was carried on under such conditions of laboratory control as to warrant the conclusions reached. The firing tests conducted have not been accompanied by laboratory investigations that are considered essential in industrial investigations of such problems.

Several years ago Frankford Arsenal tested the effect of chrome plating of small arms. Highly erratic results were obtained. Laboratory methods were not used in the work. Later a larger gun was bore plated and tested at the Proving Ground and unfavorably reported upon. The Navy has developed chrome plating of bores with beneficial results. In the past few years marked progress has been made in the development of plated layers. These developments and the test of platings of other metals have not been applied to guns.

Some experience in the effect of chamber design upon rates of erosion has been accumulated as a result of changes in gun construction. No controlled programs of study are known to have been made.

The reduction of erosion through cooling of the gun has been applied in various designs. Water cooling has been most successful but is not applicable to many types of guns and is highly undesirable in others. The use of radiating fins has also been tried. Various designs have been made with generally unsatisfactory results. In no case has a satisfactory metallurgical joint been provided between the gun and the radiating surfaces. Other methods of cooling as an induced air flow have been tried. It is believed that something might be accomplished provided some new materials and processes available were experimented with.

Some experience has been gained as to relative effects of different powder compositions on erosion. Likewise general knowledge is available as to rates of wear of guns of various calibers and muzzle velocities.

The Department has recently made a contract with Battelle Memorial Institute for erosion studies. It is understood that the objectives cover the protection of guns against erosion rather than a study of the physics of the problem. The sum allocated for the work is too small for any important results. The Institution possesses personnel and facilities for an intensive study if financed by the Department.

In conclusion, no investigation has been made or is in progress which will give a real understanding of the mechanism of erosion, nor a practical solution of the problem. Such knowledge is needed for an intelligent study of methods of obtaining longer service lives of guns.

No carefully prepared programs have been organized and carried out using modern research methods to improve gun design and to specify metals and the processes of production. Industry cannot be expected to solve these problems nor to furnish metals most resistant to erosion upon specifications based upon physical properties.

## Prospects of Future Work

It is my opinion that the advancement of laboratory methods and better knowledge of materials and of design of structures and the unsatisfactory performance of guns warrants the expenditure of funds required. The cost of laboratory and experimental work will be a fraction only of the possible savings obtained in in-

creasing the accuracy life of guns. The sum made available at this time should be large, judged from the standpoint of funds expended in research.

The most fertile fields of investigation are: An investigation of the mechanism of erosion, study of platings or other bore surfacing layers, study of designs and metals to secure a more rapid dissipation of heat from the bore, study of designs to lessen the scouring action of powder gases and study of improved metals and methods of processing to withstand erosion.

Any adequate prosecution of such a program involves the services of several laboratories and the use of engineering designs quite different from those now employed. Laboratory methods of study independent of current production work are essential. After laboratory results are obtained the application of conclusions becomes a matter for the design and production engineer.

There are a number of laboratories with personnel well trained to handle various phases of such a project.

# APPENDIX

COUNCIL OF NATIONAL DEFENSE

1530 P Street, N.W.
Washington, D.C.

5 August, 1941

Dr. R. C. Tolman,
Chairman, Division A,
National Defense Research Committee,
c/o National Research Council,
2101 Constitution Avenue,
Washington, D.C.

*Attention Dr. E. C. Watson*

Dear Doctor Tolman:

For your information, not as a formal report, I should like to indicate the current status of the erosion problem and the direction in which we are proceeding. It will be recalled that this project under the title "Erosion and Special Factors in Gun Design" was set up as a long-range problem, it being understood, however, that the testing of devices or methods for immediate application should not be excluded; and that the importance of the projected investigation was judged to depend upon the fact that whatever may be the significance of erosion studies per se, a reduction in the rate of erosion is necessary for the attainment of higher muzzle velocities because erosion is the limiting factor at present in attaining muzzle velocities much higher than are used in ordinary guns.

*Preliminary Survey of Field.* The present stage of our efforts is one of gathering information. It soon became evident that there exists a formidable quantity of factual material giving results of experiments and experiences on erosion in guns. Much of this material is to be found in the library of the Ordnance Department — in books, journals and arsenal reports. There is also in that library an extensive bibliography of the subject.

Although it was obvious that a comprehensive summary of existing information would take many months to prepare, it seemed that a survey of selected parts of the field could be made in a reasonably short time. Dr. John S. Burlew, of this laboratory, is now spending full time at the Ordnance Department in preparing a preliminary report on selected topics. He will obtain the best answers possible at this time to the following questions:

What are the pressures and temperatures in guns of representative sizes as a function of time and of distance along the barrel?

What is the composition of the gases (actual rather than theoretical) produced by the common propellants?

What types of steel are used in guns and liners of representative sizes?

What is the usual composition of the band; what are its physical properties; what amount of leakage ordinarily takes place past the band; and what is the effect of such leakage?

What are the main factors of erosion, e.g., representative amounts at muzzle and breech, relative effects in lands and grooves, nature of altered layer on inside surface?

In general, what factors promote or hinder erosion as regards temperature, pressure, type of powder and type of steel?

In our search for basic information we have had the willing and unreservedly friendly co-operation of the Ordnance Department members; for example, General Somers, Colonel Dix, and especially Major Zeller who was assigned by General Somers to aid us in finding and correlating the data from various sources. Major Zeller has also been of great service in making arrangements for visits to arsenals and proving grounds where a first-hand view of matters relative to ordnance has been and is being obtained by the undersigned and by members of our staff.

Dr. Fred E. Wright, of this Laboratory, by having a Reserve Commission in the Ordnance Department, and through knowing intimately many Ordnance Officers, has been able to expedite our initial efforts. In conformity with what was deemed preferable by the Division Chairman, we have thus far been obtaining our information from Army sources, leaving Navy contacts to follow the preliminary stage of our survey. Proper steps have already been taken to obtain pertinent available information on erosion from British and Canadian sources.

Realizing the necessity for a knowledge of the facilities of the various research laboratories and the kinds of work that are being or could be undertaken, the undersigned is having profitable conferences with various persons who have experience in directing or advising upon large research programs. Among those whose connections and ideas have already made contact advantageous are J. H. Critchett, Director of Research, Union Carbide and Carbon Corporation; John Johnston, Director of U.S. Steel Research Laboratory; C. E. MacQuigg, Dean of Engineering, Ohio State University; and Earl P. Stevenson, President, A. D. Little, Inc. Contact with research departments of the Chrysler Corporation will be made within a few days, and a little later with organizations such as General Electric, Westinghouse, Bell Telephone and General Motors.

In order to obtain detailed information, more especially on apparatus and technique for determining the structure of metals and the nature of surface films by X-ray and electron diffraction methods, Dr. George Tunell, of this Laboratory, is now making an extensive trip during which he will visit Toronto, Sudbury, Quebec, Cambridge, Schenectady and New York.

It is difficult to estimate at this time how much longer it will take for our initial survey of the field, but probably by early in September we shall have a useful summary of past work and present facilities.

*Nature of Problem.* It is obvious that in order to minimize erosion we need to understand the mechanism. This involves a whole series of subsidiary problems in chemistry, physics and metallurgy. The rate of erosion depends upon numerous factors that have been suggested and possibly on other factors that have as yet not been connected with the problem.

It is interesting to note the statement by Colonel Jenks in a memorandum entitled "Erosion, Current Status" to the effect that no investigation has been made or (at the date of writing) was in progress which would give a real understanding of the mechanism of erosion, and that no carefully prepared programs

have been organized and carried out using modern research methods to improve gun design. Colonel Jenks also predicted that any adequate prosecution of such a program would involve the services of several laboratories.

*Methods of Attack*. It is too early to outline a program even in its broad subdivisions or to specify how the necessary work can be distributed most effectively among the Geophysical Laboratory and other research laboratories; nor is it possible yet to evaluate completely the various opinions on basic questions such as, for example, whether erosion is caused mainly by the melting of the surface layer, by alteration and wearing of this layer, or by abrasion due to unburned powder grains; whether leakage past the rotating-band is important; and the nature of the altered layer.

Some facts, however, do stand out at the present time; for example, erosion is most pronounced at the breech just in front of the powder chamber; a potent factor is the length of time to which the metal surface is exposed to the hot gases; alloys containing nickel show pronounced effects; and some sort of correlation exists between the rate of erosion and the behavior by certain corrosion tests.

We know in a general way, however, that we shall have to make controlled experiments on erosion, measuring all the pertinent factors such as temperature and pressure of the gases; the structure of the altered solid layers, and the surface activity. It will undoubtedly be necessary also to make experimental and theoretical heat transfer studies. Solid diffusion is another outstanding factor. Many specific suggestions have already been made and will need to be evaluated preparatory to trying out the promising ones.

Some of these suggestions are as follows: Probability that steel under considerable compressive stress will be less reactive to hot gases; methods and devices for obtaining this state of compressive stress; possibility that formation of iron carbonyl is an important factor in gun erosion; the use of mass spectrograph for analyzing gaseous products of combustion; tapered grooves and other devices for reducing the effect of gas leakage; use of electron diffraction back-reflection methods for studying surface films on metals; use of spectroscopic methods and specifically the reversal effects for determining instantaneous temperatures of gas mixtures; and testing of the effects on metals of hydrogen dissolved in a liquid under hydrostatic pressure.

* * * * *

An item for early consideration is the formation of a strong section for the final determination of the lines of attack and the choosing of the places where the investigations are to be carried out. Suggestions for section members can probably be made soon. It is important that one or more members should have a wide knowledge of the metallurgical field because the problem of erosion, although touching several fields of science, involves metallurgy at almost every turn.

When further developments occur, I shall communicate them to you.

Very sincerely,

L. H. Adams
Chairman,
Section A, Division A

LHA/O.

# APPENDIX 4

COUNCIL OF NATIONAL DEFENSE

1530 P Street, N.W.
Washington, D.C.

October 17, 1941

Dr. R. C. Tolman, Vice Chairman,
National Defense Research Committee,
2101 Constitution Avenue,
Washington, D.C.

Dear Dr. Tolman:

I am now prepared to make recommendations as to Members of Section A.

Dr. E. R. Weidlein, Director of the Mellon Institute, Pittsburgh, Pennsylvania, is almost uniquely qualified to aid in carrying out the kind of program that is before us. He probably knows more than any other individual concerning what is going on in chemical, physical-chemical, and chemical engineering research in this country both on the academic and the industrial sides. He is energetic and capable of taking numerous responsibilities without slighting any of them. In the course of an informal conversation he indicated that he would be very glad to accept Membership in the Section and to devote a reasonable amount of time to the organization and continuing appraisal of the program. As a Consultant to Division B, he has already acted in an advisory capacity on matters submitted to him from time to time, but for Dr. Conant's benefit I should like to make clear that there need be no fear that the new assignment would conflict with his other connection with the NDRC.

Dr. Lyman J. Briggs, Director of the National Bureau of Standards, also has indicated in conversation that he will be glad to serve as a Section member. His wide acquaintance and contact with many fields of research and with the organizations conducting research will make him a valuable addition.

Not long ago I discussed with Dr. Bush my proposal to make Weidlein and Briggs Members of the Section, and he expressed his approval with respect to these two names and also one or two others that were mentioned at the time. The two now suggested will certainly fulfill the qualifications for section members that Dr. Bush has had in mind, one of which is to add "weight" to recommendations concerning program and allocation of funds. I took the liberty of asking the opinion of Dr. Weidlein and also of Dr. Briggs as to what additional members we should have. They both felt that there should not be more than a total of five members, including the chairman, and they promised to give consideration to the matter and to make suggestions as to one or two additional members.

I hope it will be possible for the suggested appointments to be made at an early date.

Very sincerely yours,

/S/ L. H. Adams
Section Chairman

LHA/jao

# APPENDIX 5

July 6, 1944

TO: Dr. L. H. Adams
Dr. H. B. Allen
Dr. W. Bleakney
Dr. L. J. Briggs
Mr. E. L. Rose

FROM: E. R. Weidlein, Chairman, Committee on Organization,
Division One, NDRC

As a result of numerous telephone conversations, telegrams, and exchange of letters, there does not seem to be any common ground of understanding, and even personal conference between members and assistants when expressed in writing do not agree with the decisions arrived at in personal meetings as to the type of reorganization necessary to make the activities of Division One more effective. Therefore, I am taking it upon myself to express my understanding of the type of organization required when I accepted the Chairmanship of this Committee.

Division One must have an active chief and he should be supported by Division Members of his own selection. This man should be Dr. L. H. Adams, as he has built up the Division and knows every phase of its development.

Division One has expanded so much and many problems have now passed from the research and experimental stage to the more advanced engineering stage that Dr. Adams needs a right-hand man as deputy chief to assume responsibility in carrying out this second stage. Dr. H. B. Allen is well qualified for this task. He has obtained permission from the Franklin Institute to give the necessary time and, also, the Institute has promised to provide space to house the organization so that Dr. Allen can be in constant contact with the work. This expansion will require a shift of personnel from Washington to Philadelphia, which I am sure can be worked out satisfactorily between Dr. Adams and Dr. Allen. Also, an additional staff of new employees will be required at Philadelphia. These people should be selected by Dr. Allen but should have the approval of Dr. Adams. The number of men to be selected, the financial obligation involved, the expense in providing quarters at Franklin Institute, and the over-all general operating expense should be approved by the Division Members as well as the Chief. The organization should be established the same as any well run institution; with a Board of Directors, consisting of the Division Members, a President, who would be the Division Chief, and an operating head who would be the Deputy Chief.

The Chief, being the designated contractual ARCO[1] and Scientific Officer, as described in Dr. Bush's memo of May 24, 1944 ("except in cases of conflict of interests") will retain the responsibility for performance of contractors. He can delegate as much authority as he desires to the Deputy Chief to see that contractors carry out their obligations in line with the prescribed programs, or to recommend new development, and if necessary, to recommend new contractors.

[1]Authorized Representative of the Contracting Officer.

A technical staff to administer the technical work of the contractors will be required, as well as a business staff to administer the business aspects of the contracts. The Deputy Chief may also contact the Divisional Committees or members of the Division for advice and assistance. In case of "conflict of interest," the Deputy Chief will be the designated ARCO and SO.[2] He may delegate authority on such contracts. However, the final responsibility of certification of performance to the Government can only be performed by the contractually designated ARCO and SO.

The appointment of Members of the Division will remain the direct responsibility of the Chief; he, however, looking to the Deputy Chief for advice and recommendations. The Division Members should be members of various reviewing committees in order to effectively carry out their obligations and properly advise and support the chief.

As previously stated, the Chief and Deputy Chief should select their immediate assistants, the number and financial obligation to be approved by the Division Members.

The Chief retains direct decision as to the disposition of inventions and may also retain direct supervision of certain fundamental research, if he so desires.

The responsibility for keeping the Chairman's Office informed of the acts of the Division and for referring to the Chairman's Office matters which are unusual from the standpoint of their nature or scope, and which thus raise questions as to whether they are in accord with the program and policies of NDRC, will be decided between the Chief and Deputy Chief for specific instances as they desire.

The only necessity for any change in administration of Division One activities at this time is on account of the volume of work, the necessity to develop the engineering side of the problems, and the desire to give more co-ordinated and direct supervision to the individual contractors, in order to obtain definite results in the shortest possible time.

The manner in which the Deputy Chief assumes the responsibilities delegated to him by the Chief and how he sets up his organization to carry out these new duties should be left entirely in his hands, except that he should keep the Chief and the Division Members informed of the type and scope of his organization and activities.

It is understood that the delegation of this authority from the Chief to the Deputy Chief and the establishment of the branch headquarters at the Franklin Institute has the approval of the Chairman's Office. The main headquarters of the Division will still be maintained in Washington.

The call for meetings will be arranged between the Chief and the Deputy Chief, and the Chief may grant authority for the Deputy Chief to call committee meetings at his will. But the results of such committee meetings should be reported to the Chief.

In other words, the Chief and Deputy Chief will act as a team but will divide the responsibilities as outlined. Both will be kept fully informed of the Division activities and the Chief will retain the responsibility for the performance of the Division.

[2]Scientific Officer.

# APPENDIX 6

Office for Emergency Management
NATIONAL DEFENSE RESEARCH COMMITTEE
of the
OFFICE OF SCIENTIFIC RESEARCH AND DEVELOPMENT
DIVISION ONE

4 September 1944

Dr. E. R. Weidlein
Mellon Institute of Industrial Research
Pittsburgh, Pennsylvania

Dear Dr. Weidlein:

The purpose of my letter of 26 August, was merely to suggest that if the Organization Committee wished to make additional recommendations on any phase of our Division affairs, it is privileged to do so. Subsequent to our last Division meeting, I have given further thought to the whole subject of organization, particularly with reference to the relation of Division Members to the management of our broad program.

It is especially important at this time to review the status of Members of an NDRC Division and to have a clear understanding of the general functioning of Division Members as a group and of the relation of this administration of the program. The duties and responsibilities of Division Members have been formulated definitely, although not in great detail, by various NDRC Memoranda such as Office Memorandum No. 42 by Irvin Stewart dated 23 September 1941, Administrative Memorandum from E. L. Moreland dated 23 November 1942 and Administrative circular by E. L. Moreland dated 7 June 1943. These duties are principally as follows: (1) to prepare for research and development by formulating appropriate programs; (2) to consider specific contract proposals and to stand squarely behind such allocation of Government funds as may be recommended by them; (3) to maintain such contact with the progress of the work to insure the most effective outcome; (4) to formulate matters of policy relating to Division activities; and (5) in general to advise the Division Chief, who is specified as the "administrative and executive officer of the Division (with power to delegate most of his functions)," and is made responsible for the administration of the Division in accordance with the rules of NDRC and with "the general guidance and any specific emphases recommended by the Division Members."

The structure of Government within NDRC may be considered to involve three main classes of functions — legislative, judicial and executive. These activities overlap to a certain extent — certainly more so than in the case of the U.S. Government itself — but such a division of activities may be useful for purposes of the present discussion. Broadly speaking, legislative actions may be considered within the province of NDRC and OSRD.

A Division as a whole (although its area is difficult to label explicitly) may be

considered to function primarily in a judicial capacity, while the executive function is exercised by the Division Chief and the persons to whom he delegates the necessary authority. One instance of the overlapping of functions in the recent past is afforded by our Steering Committees, which originally were Reviewing Committees but later took part in the management of the respective programs under the jurisdiction of the several committees. On the whole this has worked very well, but it may be doubted whether in the new compact administrative set-up there will be any need for Steering Committees, as such, with the present combination of duties. The necessity for reviewing, reporting and the presentation of specific recommendations at Division meetings will, of course, continue.

As I understand it, Dr. Allen will establish as a part of the administrative framework under his purview a number of control-committees for the several fields of activity, these committees having various purposes including assisting in carrying out the respective programs, and being made up of (a) Members of the administrative staff, (b) Contractors' representatives, and (c) a Division Member. The first may be considered the primary class; and I have endeavored to make it clear that Dr. Allen has a free hand in making his own arrangements for the organization of the staff. As for class (b), I have already communicated to Dr. Allen some remarks relative to the need for clarification of relationships. The Division Member representation on such an administration committee, however, deserves separate consideration. Possibly, the particular Division Member could act as the Reporter for a given group of projects at Division Meetings, but this merits careful thought in advance of making a decision not to have a separate reviewing mechanism. If the Division Member's connection with the control-committee is merely for the purpose of obtaining an intimate view of the work in progress, then well and good. If, however, he assists in the management of the projects, then to have him pass judgment, for the benefit of the Division as a whole, on work that he himself helps direct might be considered to constitute an administrative anomaly. To be sure, our present Steering Committees operate in some such fashion, but they are not tied so definitely into an administrative framework as they would be under the reorganization. There may be no problem here, but I do believe that this is one of the things that should be determined now rather than later.

Another way of looking at the matter is from the standpoint of reporting channels and lines of authority. In Dr. Allen's set-up, he will make such arrangements as seem desirable to him. Also, with few (or, I hope, no) exceptions, the channel of communication, as far as the administrative staff goes, will be to Dr. Allen and thence to me. Furthermore, for all of the more usual activities, the Contractors will deal directly with Dr. Allen or with those among the staff whom he designates to act for him. So much for the basic pattern of management. In addition to this, the reporting channel of Division Members obviously needs to be specified. My thought had been that the Division Members, according to normal NDRC practice, would report directly to the Division Chief, this not necessarily conflicting with their acting in any observing or informally advisory capacity that might be desirable. However, this whole subject is wide open for discussion as far as I am concerned.

I trust that the Organization Committee will communicate any recommenda-

tions that it sees fit to make on the general functioning of the Division as contrasted with the management of operations. The principal matters for adjudication center around the question, "what is to be the reviewing and reporting mechanism for purposes of Division Meetings." The views and suggestions of your Committee will doubtless enable us to have a clear-cut understanding in advance as to procedure and responsibility, so that the new organization can operate smoothly and effectively in all respects and can thus derive the greatest benefit from our important developments.

Very sincerely,

L. H. Adams
Chief, Division One

LHA:MG(des)

cc: H. B. Allen
E. L. Rose
J. A. TenBrook

# PERSONNEL INDEX

The names on the following lists are not included in this index: Personnel of the Allegany Ballistics Laboratory (pp. 217–218), Personnel of Other Section H Contractors (pp. 219–220), Personnel at California Institute of Technology (pp. 221–225), and Contractors' Personnel (pp. 425–445).

# SUBJECT INDEX